THE PHYSICAL SOCIETY OF LONDON.

PROCEEDINGS.

VOLUME 38.—PART 1.

DECEMBER 15, 1925.

Price to Non-Fellows **6s.** *net, post free* **6/3.**

Annual Subscription, **30/-** *post free, payable in advance.*

Published Bi-Monthly from December to August.

LONDON:

FLEETWAY PRESS, LTD.,
3-9, DANE STREET, HIGH HOLBORN, W.C.1.

1925.

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I.—THE INFLUENCE OF STRAIN ON THE THOMSON EFFECT.

By Hugh E. Smith, *B.Sc.*, Research Student at Birkbeck College.

Abstract.

The influence of tension on the Thomson effect in wires has been investigated by Nettleton's method. For pianoforte steel, charcoal iron, constantan and nickel, the Thomson coefficient numerically decreases with tension until the elastic limit is reached, after which it increases. With removal and restoration of tension a new definite cycle is followed. The Thomson effect in tungsten, here measured for the first time, increases with tension and also follows a cycle. No change in the Thomson effect is noticed in brass or German silver. Heterogeneity was not present in the specimens investigated.

Introduction.

CONSIDERABLE attention has been given of late to the effect of strain and torque on the thermal and electrical conductivities of metals, and in May, 1923, Dr. H. R. Nettleton—under whose guidance this research has been carried out—suggested to the writer the probability that strain and torque would cause an alteration in the value of the Thomson effect. Accordingly preliminary experiments were made in the summer of 1923 by reversing the heating current in a wire of constantan passing through two copper blocks and thereby observing the relative deflections on a sensitive galvanometer due to the change in resistance of two detecting coils situated respectively at the quarter and three-quarter positions between the isothermal copper ends and connected to adjacent arms of a Wheatstone Bridge. Such changes in temperature are proportional to the Thomson effect divided by the thermal conductivity of the wire. The experiments were of value in showing in the first place that as the effects of tension and torque on σ/k were very small, the absolute method of measuring σ would have to be pushed to the extreme limits of sensitivity; in the second place that exceptional care would have to be taken in the fixing of the wire through its ends. In view of the difficulty of the work—all measurements having to be taken between 10 p.m. and 7 a.m.—it was found advisable to limit the work to the effect of tension at one temperature only.

Method and Apparatus.

The method of measuring the Thomson effect is that fully described by Nettleton,* and recently used by Young.† Nettleton has shown that if a wire of sufficiently small length be fixed between isothermal-equipotential ends maintained at temperatures θ_1 and θ_2 respectively and transversed by a current C_1, it is possible to reverse and raise the current to a value C_2 without producing any change in the temperature of the wire. Under such conditions, with justifiable accuracy

$$\sigma = R(C_2 - C_1)/J(\theta_2 - \theta_1),$$

where

σ = Thomson effect at a temperature $(\theta_2 - \theta_1)/2$,

J = number of joules equivalent to one calorie,

R = actual resistance of the wire.

R and $C_2 - C_1$ may be measured with great accuracy.

* Proc. Phys. Soc., Vol. 29, p. 16 (1916), and Vol. 34, p. 77 (1922).

† Proc. Phys. Soc., Vol. 37, p. 145 (1925).

Though this method together with its circuit has been adhered to, considerable modifications have been found to be necessary in the actual apparatus used in view of the fact that tension has to be applied ; such modifications are described below.

The isothermal-equipotential ends each consisted of two similar blocks of copper (*P* and *Q*, Fig. 1), having a hole *A* bored in the plane of section *XX* of slightly less diameter than that of the wire it was to take. Parallel and close to this hole were larger bores *B*, which could be connected to one another by means of external copper pipes for steam or water heating. There was also a third hole *C* for the potential lead, which could be clamped by means of a copper screw, and a terminal *D* for the heating current. The two sections *P* and *Q* were clamped together by four screws and bolts, thus squeezing the experimental wire tightly in its hole *A*.

Tests were made to show whether these ends were truly isothermal. Currents up to 5 amperes were run for some time in a dummy wire fixed between them and

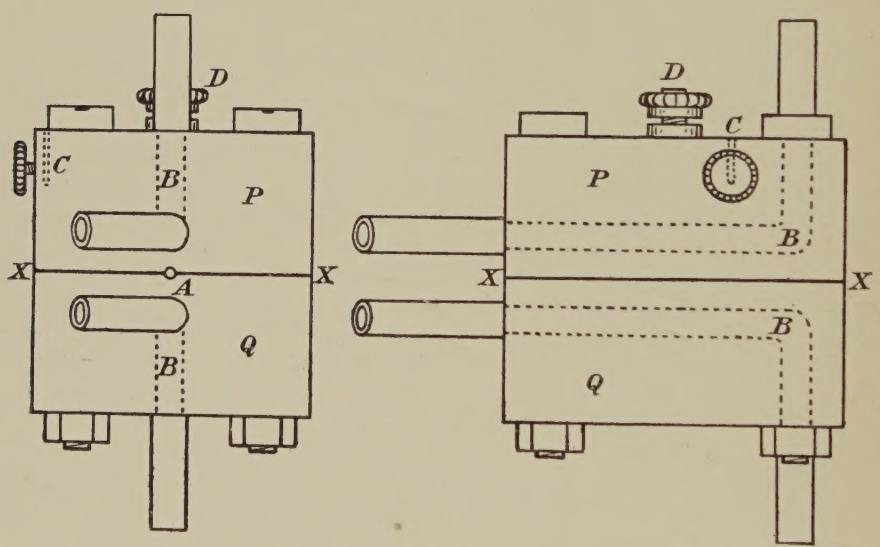


FIG. 1.

the thermal electromotive force across the ends balanced on a bridge. The current was then switched off and the fact that no change in the balance occurred showed that any heating at the ends was at once entirely dissipated. In this work the heating current run in obtaining the value of C_2-C_1 was only 2 amperes and 1·0183 amperes for the determination of *R*.

The wire—one end of which was fixed—was capable of being placed under tension by means of a differential screw with a 200 lb. spring balance attached to the other end (*see* Fig. 2). A movable platform on rollers supported the heavy copper ends, thus preventing them from bending the wire. The detecting coil, of rather more than one-third the length of the operative part of the wire, consisted of two layers of non-inductively wound silk-covered copper S.W.G.44, which was coated with an insulating enamel to prevent displacement. To prevent any progressive zero shift of the galvanometer spot, due to the warming up of the two layers

of wound wool and cotton-wool surrounding the wire and coil, an external compensating coil, suitably thermally lagged, was used; in later work, however, the best device was found to be a coil of a single turn wound directly upon one end of the detecting coil, with their leads brought out side by side, embedded in cotton-wool. The external compensating coil could be used in conjunction with this, but experience showed it to be unnecessary.

Until the later experimental stage large capacity accumulators were not

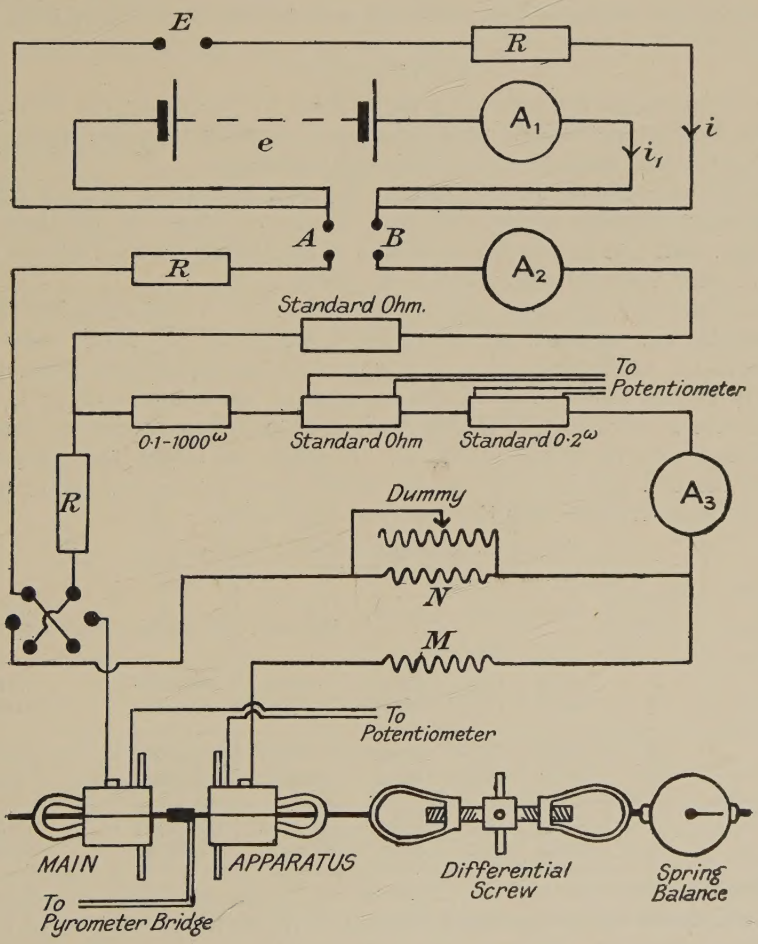


FIG. 2.—*R*, RHEOSTATS. A_1 A_3, MILLIAMMETERS. A_2, AMMETER. *M*, *N*, OHM RESISTANCES.

available, accordingly an arrangement was adopted whereby the constant heating current was taken from the 100 volt mains *E*. (The circuit is shown in the upper part of Fig. 2.) Low capacity accumulators *e* were placed in a parallel loop of negligible resistance (r_1 say) to give a slight current which rose or fell as the mains correspondingly fluctuated, thus maintaining a constant potential difference across

$A\ B$, and causing a steady current to flow through the main circuit of resistance x say. Applying Kirchoff's Laws we obtain

$$C=(e+Er_1/R)/(r_1+x+r_1x/R)$$
$$=e/x \text{ approximately, since } r_1 \text{ is small.}$$

Also by assuming likely variations of the mains E it may readily be seen that the current in $A\ B$ remains very nearly constant even when r_1 is fully taken into account, providing, of course, it is very small. This arrangement in use was found to be quite satisfactory when a steady constant current was required for long periods of time.

HETEROGENEITY.

The necessity for interchanging the heaters in measuring the Thomson effect was first noted by Le Roux,* who attempted to show that the mean value obtained was the true measure of the Thomson coefficient.

Young† has recently found that in the case of carbon steels a considerable systematically different value of the Thomson effect is obtained by the interchange, which he has shown to be due to the Peltier effect produced inside the wire consequent upon its asymmetrical structure.

When this work was first commenced the result of interchanging the heaters was invariably investigated, but as no effect was found the writer grew to ignore its probability. On learning that Young was encountering this effect, the writer made very careful studies and it was found that less than $\frac{1}{2}$ per cent. difference occurred, save in one piece of the steel wired used. This difference, which is here given, disappeared on strain (Table I). It might be added that this particular piece was the end and last of that particular specimen of steel wire, which fact seems to be in accordance with the experience of Young.

TABLE I.—*Heterogeneous Steel Specimen.*

Tension $\times 10^9$ Dynes/cm.^{2}	Heater Hot.	θ_2	θ_1	$\theta_2-\theta_1$	C_2-C_1 in amps.	R in ohms.	$\sigma_{55\cdot5}\times 10^{-6}$ Calories/coul.-deg.
0 (virgin)	A	99·99	11·40	88·59	0·08060	0·009765	$-2\cdot12_5$
	B	99·98	11·55	88·43	0·06160	0·009767	$-1\cdot62_8$
12	A	99·98	11·58	88·40	0·06157	0·009809	$-1\cdot63_8$
	B	99·98	11·58	88·40	0·06160	0·009809	$-1\cdot63_9$
0	A	99·69	11·90	87·79	0·06114	0·009791	$-1\cdot61_5$
	B	99·69	11·20	88·49	0·06111	0·009786	$-1\cdot60_1$

It is worthy of note, however, that whilst Young experienced a change when the wire was under the temperature gradient of its heating current—the ends being at the same temperature—the writer has not found this to occur. (The heterogeneous specimen mentioned above was not tested in this way before strain.) On occasions, when a new wire has been put in the ends to replace an old one for testing purposes without first obtaining a fresh surface of contact in the ends, it has been found to yield inaccurate results. This was put down to the fact that a film of oxide due to the heat had occurred at the points of contact of the isothermal ends and the wire.

* Ann. de Chimie et de Phys., Vol. 10, p. 258 (1867).
† Loc. cit.

This is only one case in point, but it was also found that unless exceptional care was taken, inconsistent results were easily obtainable as a result of bad ends. Thus it seems possible that heterogeneity, real and pseudo, may easily occur.

In all the results given below heterogeneity was absent in the specimens within the limit given—i.e., a half per cent. This is of extreme importance, as in the steel result given below (Fig. 3) the greatest change in the Thomson coefficient, due to a difference of 200 lb. tension on the 18-gauge wire, is only about 4½ per cent., whereas heterogeneity was experienced by Young in steel at this temperature, causing up to 50 per cent. change in the Thomson effect. In fact, the largest percentage change observed, which was in tungsten, was only about eight, for the maximum tension applicable.

Results.

The work was handicapped in connexion with certain metals, notably constantan and nickel, which commenced to flow under high tension, the wire slipping in the ends after a certain tension had been reached, and thus preventing further readings being taken. Slip could at once be detected by the accelerated rise in the resistance, which still further increased when the tension was removed.

Measurements were carried out for pianoforte steel, charcoal iron, constantan,

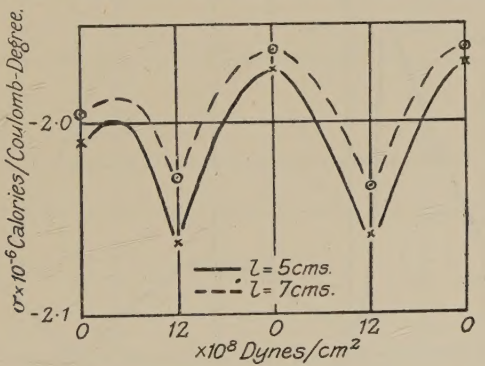


Fig. 3.—Pianoforte Steel.

nickel, tungsten, German silver and brass. The operative lengths of the wires were from 4 to 5 cm., according to their thermal conductivities. The results for those metals which showed a change in the Thomson effect are given in graphs with σ plotted against the tension, and tables are given of those which remained constant.

An example is given in the case of steel (Fig. 3) showing the effect of increasing the operative length of the wire in an attempt to gain sensitivity. It will be seen that an increase of 2 cm. on 5 cm. at once invalidates the assumption that emission losses are negligible.

It will be noticed from the graphs of steel, iron and constantan that a virgin or unstrained wire gives quite different results from one which has already been subjected to tension. In these virgin wires the Thomson effect NUMERICALLY decreases with tension until the elastic limit is reached, at which point it then increases. After a maximum strain has been reached, it then follows a definite cycle with the tension, decreasing as the tension is lessened, and increasing as the tension is restored. Several cycles were made, and were found to be quite definite. Nickel also behaves in a

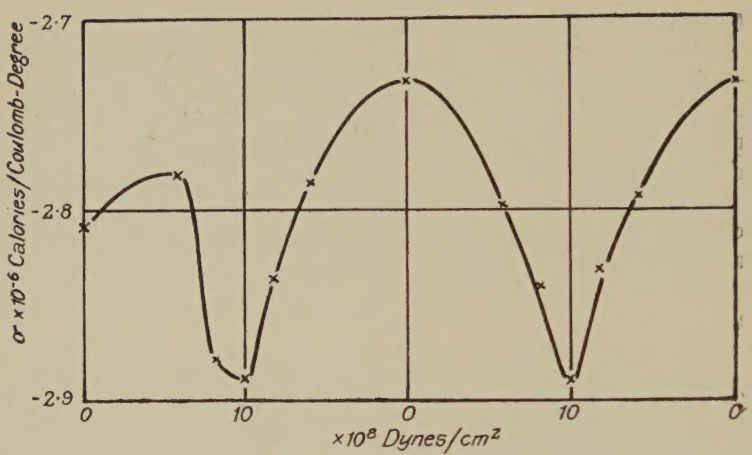


FIG. 4.—CHARCOAL IRON.

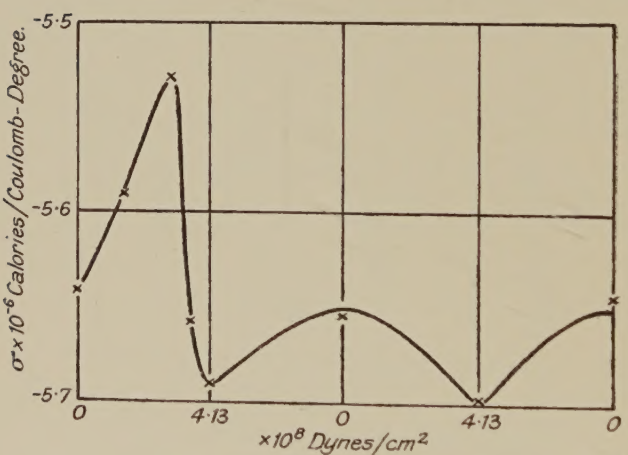


FIG. 5.—CONSTANTAN.

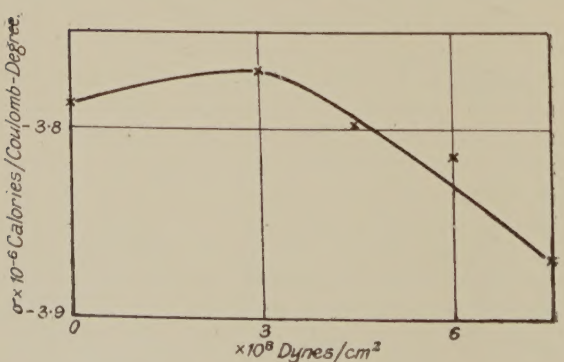


FIG. 6.—NICKEL.

like manner, but unfortunately cycles could not be proceeded with owing to the wire flowing and thus slipping in the ends, as has been mentioned above.

The Thomson effect in tungsten has never previously been measured, except at incandescent temperatures. For this reason a short table, as well as a graph, is given here (Table II). The specimen was 99·95 per cent. purity; the

TABLE II.—*Tungsten.*

Tension $\times 10^9$ dynes/cm.^{2}	θ_2	θ_1	$\theta_2-\theta_1$	C_2-C_1 in amps.	R in ohms.	θ	$\sigma_\theta \times 10^{-6}$ Calories/coul.-deg.
0 (virgin)	99·49	11·90	87·59	0·24156	0·003089	55·7	$+2·03_8$
2·95	99·47	11·92	87·55	0·24618	0·003090	55·7	$+2·07_9$
5·90	99·47	11·98	87·49	0·24905	0·003091	55·7	$+2·10_5$
8·85	99·47	11·98	87·49	0·25493	0·003103	55·7	$+2·16_3$
11·80	99·47	12·08	87·39	0·26022	0·003119	55·8	$+2·22_2$
0	99·47	12·12	87·35	0·26024	0·003089	55·8	$+2·20_1$

impurities being Fe 0·014 per cent., Mn 0·010 per cent., Ca 0·007 per cent., Ni 0·0052 per cent., with a trace of silicon. This was the only metal of positive Thomson

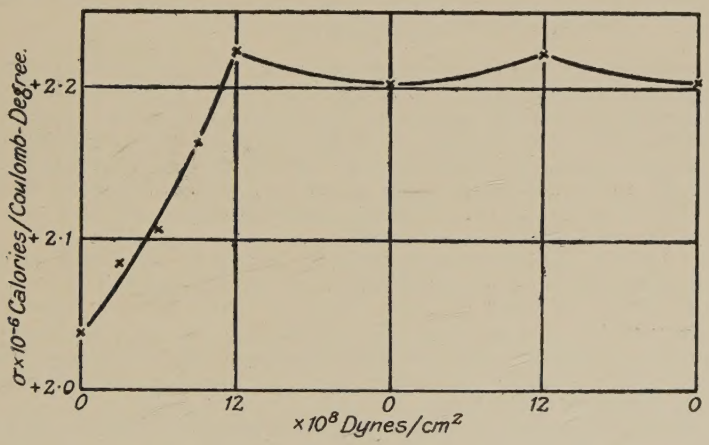


FIG. 7.—TUNGSTEN.

coefficient investigated; and, due to the limitation of the tension apparatus available, it was impossible to exceed the elastic limit. However, the metal behaved in a similar manner to those given above, the Thomson coefficient increasing with tension and following a definite cycle on removal and restoration of the strain.

TABLE III.—*German Silver.*

Tension $\times 10^9$ dynes/cm.^{2}	θ_2	θ_1	$\theta_2-\theta_1$	C_2-C_1 in amps.	R in ohms.	θ	$\sigma_\theta \times 10^{-6}$ Calories/coul.-deg.
0 (virgin)	100·22	9·50	90·72	0·1286	0·008454	54·8	$-2·86_6$
2·95	100·21	9·44	90·77	0·1287	0·008465	54·8	$-2·87_0$
5·90	100·20	9·50	90·70	0·1278	0·008485	54·8	$-2·86_9$
8·85	100·20	9·52	90·68	0·1277	0·008515	54·8	$-2·86_8$
10·62	100·19	9·52	90·67	0·1272	0·008554	54·8	$-2·87_0$
0	100·19	9·49	90·70	0·1288	0·008445	54·8	$-2·86_9$

TABLE IV.—*Brass.*

Tension $\times 10^9$ dynes/cm.^{2}	θ_2	θ_1	$\theta_2-\theta_1$	C_2-C_1 in amps.	R in ohms.	θ	$\sigma_\theta \times 10^{-6}$ Calories/coul.-deg.
0 (virgin)	99·40	13·50	85·90	0·02885	0·003435	56·5	$-0{\cdot}276_0$
2·95	99·40	13·50	85·90	0·02882	0·003440	56·5	$-0{\cdot}276_1$
5·90	99·40	13·50	85·90	0·02868	0·003458	56·5	$-0{\cdot}276_2$
8.85	99·40	13·50	85·90	0·02831	0·003497	56·5	$-0{\cdot}275_7$
10·03	99·40	13·50	85·90	0·02806	0·003525	56·5	$-0{\cdot}275_4$
0	99·40	13·50	85·90	0·02874	0·003452	56·5	$-0{\cdot}276_3$

Any changes which may have occurred in the Thomson coefficient in German silver and brass on the application of strain were very small and within the limit of experimental error.

In conclusion, the writer wishes to take this opportunity of expressing his gratitude to Dr. Nettleton for his great assistance and advice, and of recording his thanks to Prof. A. Griffiths, Head of the Physical Dept., Birkbeck College, for placing so much apparatus and room at his disposal for such a length of time.

DISCUSSION.

Prof. A. GRIFFITHS said that the Paper afforded a good example of what could be achieved by an evening student in particularly difficult circumstances. The author had unfortunately been compelled, while his work was in progress, to take up a post which obliged him to live in Lincolnshire, so that it had been necessary for him to travel to London night by night in order to carry out his experiments. It might be added that the acknowledgment of Dr. Nettleton's assistance was not to be understood as a formal one, for Dr. Nettleton had generously accommodated himself to the unusual conditions under which the work had been carried out, whenever his advice and help had been required.

Mr. W. A. BENTON asked whether he correctly understood the author to say that the cyclic repetitions shown by his curves applied only to specimens which had been strained beyond the elastic limit. If so, the result is a very surprising one. When a specimen has been stressed it recovers all its ordinary physical properties when the stress is removed, unless the elastic limit has been exceeded: is it to be understood that the Thomson coefficient follows quite the opposite rule? And has the author made any repeating cycles of observations for strains lying well within the elastic limit?

Mr. R. S. WHIPPLE said that the same question had occurred to him, and he would like to suggest that the author should try whether the Thomson coefficient was affected by annealing a wire which had been strained. It is easy to free a wire from all strain by this process, and quite a moderate annealing temperature will serve, a change of 50°C. being sufficient. It might be worth while, therefore, to take observations before and after annealing a specimen.

Mr. J. GUILD said that as the author had asked for suggestions, he would like to make two. (1) Possibly the difficulty which had been experienced with regard to the slipping of the specimens in the clamps could be got over by using very strong spring washers tightened up as much as possible. (2) Very fine enamelled wire can be obtained, and as this takes up much less space than double-silk covered wires and is otherwise satisfactory, it might be more suitable for the construction of the detecting coil.

Mr. F. E. SMITH said that as he disliked using thermo-couples himself, he would suggest to the author the possibility of substituting a resistance method of finding the temperature of a specimen. For this purpose it might be desirable to employ a modification of the Kelvin bridge in which, besides the extra mesh ordinarily added at the end of one of the galvanometer leads, a further mesh is added at the end of one of the battery leads. He congratulated the author on having carried through so much work under such trying conditions, and expressed particular interest in the arrangement shown in Fig. 2 for obtaining a steady current from the mains.

Mr. JAMES YOUNG (communicated): I should like to express my appreciation of the

careful piece of work which Mr. H. E. Smith has carried out on the influence of strain on the Thomson effect. I had the pleasure, some time ago, of seeing his apparatus, and I fully realise the difficult conditions under which he had to work and which he seems to have combated successfully by experimenting during the night. His remarks under the heading "heterogeneity" are of interest to me. I notice (Table I.) that he has detected an asymmetry of 27 per cent. in a specimen of steel wire, but unfortunately has not given the percentage composition of the steel. This is important because asymmetry, if it occurs, only becomes marked when the percentage of carbon in the steel exceeds a certain value (at least 0·6 per cent.). There seem to be two types of asymmetry in wires, which exhibit themselves in Thomson-effect measurements. One of these occurs only when the wire is under the temperature gradient due to the heaters; the other can be detected even if the temperature gradient is due to the current in the wire alone. It was the latter of these phenomena which I attempted to explain in my recent Paper on the Thomson effect, to which Mr. Smith refers. I have been unable to give any satisfactory explanation of the former phenomenon, which when present is generally small (about 5 per cent. in iron). The changes due to strain which Mr. Smith has detected in charcoal iron are of this order of magnitude and perhaps some explanation of this type of asymmetry in terms of strains could be given.

AUTHOR'S reply: In reply to Mr. Benton and Mr. Whipple, the cycles were made after maximum tension had been applied—that is, after the elastic limit had been exceeded. With some specimens the cycles were repeated after the interval of some days, and the values previously obtained were again recorded. This does not apply to tungsten, where all measurements were made with stresses within the elastic limit; but here again σ followed a cycle with removal and restoration of tension. This cycle was not repeated after several days. I must express my thanks for the suggestions given.

(Communicated subsequently): The suggestions that cycles be made for strains lying well within the elastic limit and the annealing of wires were considered, but had not been carried out owing to pressure of time, and also in the latter case owing to the fact that an annealed wire tends to flow more readily than an unannealed wire under high tension. I agree that experiments conducted on these lines would be of value as an addition to those in the Paper. I thank Mr. F. E. Smith for his suggestion of using a Kelvin bridge in place of the thermo-electric potentiometer. I would like to point out the following:—

1. The T.E. potentiometer allows R and C_2-C_1 to be measured under the temperature gradient with great rapidity and an accuracy greater than is required.

2. The value of the standard ohm is eliminated by using the T.E. potentiometer for R and C_2-C_1.

3. The Kelvin bridge requires additional space and apparatus.

4. Thermo-couples were not employed in this work, though they were used in the other Papers referred to in this Paper.

I greatly appreciate Mr. J. Young's remarks, but as only one specimen of the steel wire exhibited any measurable heterogeneity—which disappeared under tension—I can add no further remarks of value, as much further work is necessary in order to investigate any heterogeneity which may appear. I regret that the percentage composition of that heterogeneous specimen of steel has not been determined. It was ordinary commercial "pianoforte steel" wire.

II.—ON THE FLASHING OF CERTAIN TYPES OF ARGON-NITROGEN DISCHARGE TUBES.

By William Clarkson, *M.Sc.*, *A.Inst.P.*, Armstrong College, Newcastle-on-Tyne.

Received July 10, 1925.

Abstract.

The Paper gives an account of investigations on the flashing of discharge tubes* having various types of electrodes, and in which the filling gas, a mixture of argon and nitrogen, had a wide pressure range.

It is shown that the observations are in general agreement with those obtained previously on neon† and air‡ discharge tubes, minor differences between tubes being discussed, and that at higher pressures the discharge was no longer of the "glow" type, but took place between definite points on the electrodes.

Introduction.

THE following Paper is a continuation of previous work on the flashing phenomena of discharge tubes, in which, as the electrodes are only one or two mms. apart, the positive column is absent, and only the negative glow, with an occasional anode glow, is in evidence.

The initial experiments of flashing were confined to "Osglim" lamps, but later air discharge tubes were constructed. In the "Osglim" lamps the electrodes were of iron, and the filling gas, a mixture of neon and helium, in all cases was at approximately the same pressure, that giving the lowest sparking potential. By employing air as a filling gas, however, it was possible to vary the type of tube and the pressure of the gas at will, though actually most of the experiments were performed on converted "Osglim" lamps and on tubes having parallel molybdenum wires as electrodes, and as the critical potential for air is high, lack of suitable capacities limited the pressure range for all but small capacities to about 4 mms.

Since as much general information as possible on flashing was required, it was considered desirable to extend the investigations to more varied types of discharge tubes and over a wide range of pressures. It was essential to obtain, therefore, first of all a supply of gas giving low critical voltages. The critical potential conditions were found to be satisfied by the argon-nitrogen mixture contained in "Osram" gas-filled lamps, and a plentiful supply of this gas was made available through the generosity of the G.E.C., who provided a number of lamps whose filaments had broken.

In order to make apparent any peculiarities due to one tube, or to the material and shape of its electrodes, tubes of the following types were employed: "Osglim" lamps of both the beehive and letter patterns, tubes in which the electrodes were parallel wires, and tubes having for one electrode a helix of wire and for the other a

* *See* Pearson and Anson, Proc. Phys. Soc., Vol. 34, p. 204.

† Taylor and Clarkson, Journ. Sci. Insts., Vol. 1, p. 173; also Proc. Phys. Soc., Vol. 36, pt. 4, p. 269.

‡ Taylor and Clarkson, Proc. Phys. Soc., Vol. 37, pt. 3, p. 130; also Phil. Mag., Vol. 49, p. 336.

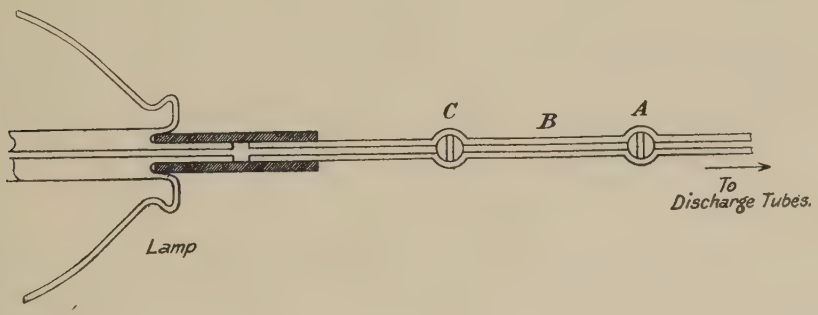
straight wire down the axis of the coil. The wires were of iron, copper, aluminium or molybdenum. In mounting, the electrodes were fixed as far from the containing vessels' walls as possible, this not only enabling the tube to become heated with safety, but also diminishing wall effects.

For the measurements of the apparent upper and lower critical voltages V_C and V_B of the tubes under conditions of continuous flashing, a new method described by Taylor and Sayce* was utilised. This method is based upon the ordinary peak voltage measurer, a valve being utilised to ensure unidirectional conductivity from a condenser which becomes charged either to the upper or lower critical voltages, as desired, when connected in parallel with the discharge tube and charging condenser.

Gas Apparatus.

A description of the method by which the gas was transferred from the lamp bulb to the apparatus may prove of interest here. Nippleless bulbs were utilised, these having a filling tube projecting into the cap at the base.

The cap being removed, the filling tube was connected to the apparatus by means of a short length of pressure tubing, and the taps *A* and *C* being opened, the apparatus,



was evacuated to its lowest limit, the connexion then being treated with sealing wax till no leaks were apparent. The tap *C* was then closed, and the tip of the filling tube was smashed with pliers through the pressure tubing connection, the sealing wax previously having been softened at this point. A coating of shellac completed the joints.

It appeared, however, that all precautions were unable to prevent a gradual leakage of air into the gas, though this was always so slight that the critical constants never varied greatly during the life of one bulb, one 200-watt lamp ensuring a supply of gas of suitable properties sufficient to fill the apparatus several times.

The gas-filling apparatus was manipulated in the usual way, the tap *C* serving to admit the relatively high-pressure gas from the bulb to the exhausted space *B*, which gas, when *C* was closed, finally was allowed to enter the discharge tube through the tap *A*.

The discharge tubes at first were "flushed out" thoroughly with the gas, and afterwards were always filled to the highest pressure desired, no additions of gas being made during any series of observations. Phosphorus pentoxide drying tubes were used.

* Not yet published.

Preparation of Tubes.

The method employed in the preparation of the discharge tubes was essentially the same as that described in previous work,* similar difficulties being encountered. A tube was considered suitable when the apparently inevitable changes in the critical voltages with time and with continued use were negligible, and when, on altering the circuit, the apparent critical voltages showed no appreciable "lag" behind their final values. All tubes, however, could not be made to fulfil these conditions, even when overrunning at red heat was resorted to, and many finally had to be rejected.

Experimental Relations.

Flashing was observed over a series of pressures ranging from 0·2 mms. to 45·0 mms., these being the limits imposed by the voltages and the resistances available. At pressures lower than some 2·0 cms. the flash was of the normal type, a glow discharge, but at the higher pressures the discharge was of the nature of an "arc," and the negative glow was rarely in evidence.

The relations given below are for the normal discharge; reference is made to the other form later.

Critical Voltage Measurements.

Except for the slight unavoidable variations mentioned above, measurements in all tubes showed that for any one tube at a constant pressure the upper and lower critical voltages for steady discharge, v_c and V_b respectively were constants, and that V_C and V_B, the apparent upper and lower critical voltages for conditions for continuous flashing, were also constant, providing the external circuit remained unaltered.

Observations on the effect of the presence of radium on the apparent critical voltages† confirmed the previous result that V_B apparently was unaffected. It was found, however, that V_C invariably was lowered, changes of from 2 to 70 volts being recorded, the magnitude of this change varying with the tube employed. As previously, the tubes least affected were those such as the "Beehive," in which the inter-electrode space was largely screened by the electrodes.

During flashing experiments the radium always was kept at the same place, touching the bulb as near to the electrodes as possible, observations demonstrating that the effect fell off rapidly with the distance of the radium from the tube. It was possible to change V_C at will by varying the distance suitably. In the daylight, when presumably the gas already was partially ionised, no effects were detected when the radium was only 40 or 50 cms. from the tube, but in darkness effects have been noticed with the radium some metres away.

Critical Voltage Variations.

Observations made on the variation of V_C and V_B with the charging voltage E, the circuit resistance R, and the capacity C,‡ showed that V_C and V_B, though apparently independent of E and R, and also practically constant for values of C down

* Taylor and Clarkson, Phil. Mag., Vol. 49, p. 336.

† Taylor, Clarkson and Stephenson, Journ. Sci. Insts., Vol. 2, No. 5, p. 154. Taylor and Stephenson, Phil. Mag., June (1925).

‡ Taylor and Stephenson, Phil. Mag., June (1925).

to about 1·0 microfarads, in general changed rapidly as the capacity was further reduced, though the magnitude of this change and whether an increase or decrease in value occurred seemed to depend on other factors beside the capacity.

The curves showing the variation of V_C and V_B with pressure were of the same general form as those obtained for air, the change in V_B was never as great as that in V_C, and was generally much smaller, in some cases V_B being the same at all pressures.

Relations for Time of Flash (T).

In all, flashes ranging in frequency from many thousands per second to one flash in two minutes were obtained, but in examining flashing relations only those frequencies which could be measured by a stop watch method were considered, the rate of flash varying in practice from about seven flashes per second to about one flash in twenty seconds, for even at this slow rate flashing was still quite regular. Only when the rate of flashing reached the order of one flash in some sixty seconds did irregularities occur, this, of course, being easily explained when account is taken of the effects of quite small fluctuations in E and V_C, and of the inevitable leakage in circuit.

In all cases, from the graphed relations it was found that the relation between T and C was linear if E and R were constant, the same relation holding between T and R when E and C were constant, the linearity being satisfied to a high degree of accuracy and the curves intersecting the axes at points near to the origin. This is in accord with the theoretical relation developed during the work on air, given fairly exactly by

$$T=CR \cdot \log_e \frac{E-V_B}{E-V_C}+\triangle C,$$

where $\triangle$ is a small constant, since the variation in V_C and V_B over the range of capacities employed was quite small.

Relations for the Critical Resistance R_c.

Though it was found impossible to obtain as complete a series of results for R_c as for the other flashing phenomena, the high conductivity tubes presenting the same difficulties as in air, in all cases where the results were repeatable it was observed that whilst R_c was only slightly affected for changes in the capacity at least for values of C greater than about 1 microfarad it varied linearly with E. The intercept (V_b') on the voltage axis was of the same order as V_B, the relation

$$R_c=\frac{E-V_b'}{\psi(V_b')}$$

being satisfied to a fair degree of approximation.

Electrode Fatigue and Other Effects.

Though the general behaviour of discharge tubes was found to be the same in all cases and to agree with theory, the critical voltage measurements revealed

the occurrence of phenomena which merited further explanation, chief of these being the anomalies in the variation of V_C and V_B with capacity (and, perhaps, pressure). Whilst it was known that wall charges and impurities in the gas could occasion abnormalities, the evidence indicated that the cause of these anomalies lay in the electrodes themselves, in which case probably films of absorbed gas on the cathode were responsible. This same reason would also serve to explain such facts as the following: That tubes often show great reluctance to adopt new values for V_C and V_B, lags of some fifteen minutes being recorded, that the values of these constants for tubes recommencing flashing after a period of disuse differed markedly from those stable values attained a few minutes later, and that even slight over-running often caused radical changes in their values. It was on account of these changes that over-running at red-heat was discontinued save in refractory tubes.

It is obvious from these results that when work of an exhaustive nature is to be undertaken, such effects as these must be taken into account before a tube can be declared suitable for such work.

The normal electrode fatigue did not occasion much trouble, save in R_c experiments, where, of course, its effects were often disastrous. Its presence was effectively demonstrated in tubes carrying a steady discharge which did not utilise the whole of the cathode, the usual state of affairs at R_c. In these cases the discharge often shifted from place to place on the electrodes, these effects being most striking in tubes with "beehives," or helical cathodes, the change being rotary and often occurring with great rapidity.*

Flashing at High Pressures.

As pressures of the order of 1·5 to 2·0 cms. were employed, the flashes grew more and more intense, being accompanied by a ringing of the electrodes, and it was observed in all tubes that violent discharge having the appearance of an arc or spark frequently occurred, this type displacing the glow discharge altogether at still higher pressures. Observations made on this form for very rapid flashing, when, of course, the form of the discharge could be observed distinctly, showed that the discharge actually took place between points, though a small cathode glow might be present.

Measurements showed that the apparent critical voltages were only approximately constant, the discharge even with small capacities apparently causing irregularities in their value by its intense nature. The discharge grew more intense as larger capacities were employed, until finally, when the energy transference was large each flash was in the form of a loud, vivid spark.† When this occurred it was seen that the value of V_B had fallen often entirely to zero, the condenser being completely discharged at each flash, but that V_C appeared to be unaffected.

This violent spark discharge was similar to a discharge occurring spasmodically in some air discharge tubes when salient points were present, it being observed that salient points tended to have the same effect in argon-nitrogen tubes.

The author wishes to acknowledge here his indebtedness to Prof. G. W. Todd, of Armstrong College, under whose supervision the work was carried out.

* Taylor, Nature, September 24 (1924).
† J. W. Bispham, Proc. Roy. Soc., A 81 (1908).

DISCUSSION.

Mr. C. C. PATERSON said that the gas used in the experiments was a mixture in which the proportion of argon to nitrogen was not very rigid. One lamp might differ considerably from another in this respect.

Dr. D. OWEN remarked that the author had omitted to say what were the values of the critical voltages he had measured : he hoped this omission would be remedied.

Mr. F. E. SMITH said he was sure the meeting would learn with deep regret that Mr. Anson, who first discovered the flashing of glow lamps, had been killed in an accident during the summer.

AUTHOR'S reply (communicated) : In reply to Dr. Owen, I would like to add that V_C ranged from 270 volts to 500 volts, the corresponding variation in V_B for normal flashing being of some 190 to 300 volts. The critical voltages had minimum values at pressures of a few millimeters.

Though curves showing the variations of V_c and V_b with the capacity and pressure were plotted in all cases they were omitted from the Paper because it was felt that a description of their general resemblance would suffice, the particular differences depending solely on the tube used, and, as Mr. Patterson suggested, on the argon-nitrogen mixture employed.

III.—ON THE VISCOSITY OF AMMONIA GAS.

By R. S. Edwards, *A.R.C.S., B.Sc., D.I.C.*, and B. Worswick, *A.R.C.S., B.Sc., D.I.C.*

Received September 4, 1925.

Abstract.

The viscosity of ammonia gas has been determined at three different temperatures by transpiring the gas through a capillary tube which had previously been calibrated with air. Sutherland's constant is found to be roughly 370, and the mean collisional area of the ammonia molecule $0{\cdot}633 \times 10^{-15}$ sq. cm.

I. Object of Investigation.

PREVIOUSLY no experiments on the viscosity of ammonia have been carried out over a wide range of temperature, and the object of this investigation was to study the variation of this viscosity with temperature, and to determine Sutherland's constant for the gas.

II. General Method of Procedure.

The method employed was that of transpiration of the gas through a capillary tube, the dimensions of which were ascertained by transpiring air. This method is essentially the same as that followed by Mr. H. Harle in his work on the "Viscosities of the Hydrogen Halides."*

The purified gas was liquified and then enclosed and allowed to evaporate. The process of evaporation could be controlled so as to establish a suitable driving pressure, the pressure difference between the ends of the capillary being measured on a U-tube gauge. The mass of gas passed was measured by the standard alkali-acid process for ammonia. The corresponding volume was obtained from density data.

III. Constant of Capillary Tube.

About twenty tubes of approximately 0·25 mm. diameter were calibrated and the most uniform chosen. That chosen was about 55 cms. long, with mean diameter 0·246 mms., its maximum variation being less than 1 per cent. of the mean diameter. The mean diameter was found by weighing a thread of mercury which occupied the length of the tube.

The type of apparatus used is shown diagrammatically in Fig. 1. Air was drawn through the capillary tube by means of the aspirator B, the volume of water collected giving, after several corrections, the volume of air passed. The end A of the capillary tube was open to the atmosphere through drying tubes containing calcium chloride, soda lime and glass wool. The spiral S_1 consisted of about 70 cms. of thin walled 4 mm. tubing. S_2 was a small spiral inserted to take up any strains produced in the assembling of the apparatus. The calcium chloride tube D was

* Proc. Roy. Soc., A. Vol. 100, p. 429 (1922).

inserted between S_2 and B to prevent moisture from the aspirator reaching back to the capillary tube. The pressure difference was read from the manometer M, which was a mercury gauge with wide arms to eliminate capillary effects. The difference of level was noted by a cathetometer reading to 0·1 of a mm.

To obtain concordant results it was found necessary to reduce "dead-space" on the aspirator side to a minimum. Further, it was necessary to ensure that the gas remained at a constant temperature after entering the capillary and during its passage to and in the aspirator. To realise these conditions it was found necessary to replace Mr. Harle's large aspirator, which gave a constant head, by a smaller vessel giving a slightly varying head. To maintain a uniform temperature the water from the jacket was circulated round the aspirator and drying tube, as shown in figure. This arrangement was extremely satisfactory. Variations in the pressure

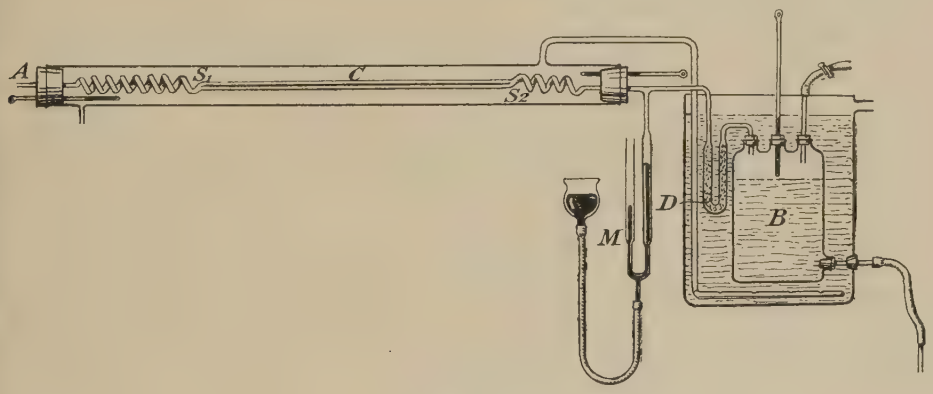


FIG. 1.

head were conveniently made between wide limits by varying the height of a nozzle attached by pressure tubing to the exit tube.

Meyer's transpiration formula

$$P_1V_1=P_2V_2=\frac{(P_1^2-P_2^2)\pi a^4}{16\eta l}$$

can be written

$$\eta=\frac{(P_1-P_2)(P_1+P_2)\pi a^4}{16P_2V_2l}=\frac{\delta(2P_1-\delta)\pi a^4 60g\rho_0}{16(P_1-\delta)V_2l}$$

where P_1=atmospheric pressure$=P_2+\delta$.

δ=pressure difference (obtained from gauge readings) measured in cms. of mercury at 0°C.

ρ_0=density of mercury at 0°C.

V_2=volume of dry air passing per minute at pressure P_2 and temperature of jacket t_c°C.

Actually P_2 decreased with the time, and a modified form of Meyer's formula should be used; but, since the variation was never more than 0·1 per cent., the assumption

that δ was equal to the mean pressure difference was quite justifiable. The volume of water collected gave the increase in the volume of saturated air in the aspirator at pressure P_2 and temperature t_a (t_a being the temperature of the air in the aspirator). The temperature t_a never differed from t_c by more than 0·1°C.

Since P_2 remains approximately constant, the partial pressure of the air in the aspirator is less than P_2 by the saturation vapour pressure of water at t_a, so that if V=observed volume of water displaced per minute we have

$$V_2=\frac{(P_2-\sigma)V}{P_2}=\frac{(P_1-\sigma-\delta)V}{P_1-\delta}$$

where σ=saturation vapour pressure of water at t.

Substituting in the equation for η we have

$$\eta=\frac{60g\rho_0\pi a^4}{16l}\cdot\frac{\delta(2P_1-\delta)}{(P_1-\delta-\sigma)V}=K\frac{\delta(2P_1-\delta)}{(P_1-\sigma-\delta)V}$$

where K is the constant of the tube.

One further correction is necessary for the value of K thus determined. This correction is for the slipping of the gas over the walls of the capillary tube.

This correction occurs as a factor $\left(1+\frac{4\varphi}{r}\right)$ in the expression for the viscosity where φ is the coefficient of slip. Thus the value of K above must be divided by this factor.

The value of φ was calculated according to the formula given by C. J. Smith,* namely,

$$\varphi=\frac{1\cdot70\eta\sqrt{3RT}}{(p_1+p_2)}$$

where p_1 and p_2 are initial and final pressures, T is absolute temperature and η is the viscosity. This correction was of the order of 0·7 per cent.

The values of $\eta_{(air)}$ used in calculating K are the most recent of Prof. Rankine—viz.,

$$\eta_0=1\cdot724\times10^{-4}\text{ C.G.S. units}$$
$$\eta_{15}=1\cdot799\times10^{-4}\quad\ldots\quad\ldots$$

All joints in the apparatus were, as far as possible, of glass, the others necessary being well sealed with collodion. This gave perfectly airtight joints.

The results were concordant up to a pressure difference of 6·1 cms. of mercury. With differences exceeding this turbulence set in.

The temperature of the jacket and aspirator was maintained constant by passing a rapid stream of water through the circuit. The jacket temperature was taken as the mean of the inlet and outlet temperatures measured with mercurial thermometers graduated to 1/10°C. These and the aspirator thermometer were calibrated by comparison with a standard mercurial thermometer.

The volume of water collected was found by weighing, corrections being applied for buoyancy and density as usual. The times were measured with a stop watch.

* Proc. Roy. Soc., A, Vol. 106 (1924).

List of Results for K.

Table I.

t°C.	$K \times 10^5$	t°C.	$K \times 10^5$	t°C.	$K \times 10^5$
12·05	6·52	12·14	6·57	11·00	6·57
12·16	6·57	11·99	6·56	12·82	6·54
11·76	6·54	11·46	6·51	12·28	6·53
11·55	6·56	11·30	6·57	12·78	6·58
11·88	6·57	11·41	6·57	1 ·71	6·57
12·03	6·58	11·58	6·57	13·82	6·57

Mean value of $K = 6{\cdot}56 \times 10^{-5}$.

Mean value of $t = 11{\cdot}87$°C.

Correction factor for slipping for air $= 1{\cdot}0067$.

The mean value of t is taken because of the small variation of K over our range of temperature.

IV. Preparation of Ammonia Gas.

The following method was adopted for the preparation of the gas. Pure ammonium chloride and slaked lime were warmed together. This gave a big yield of gas, which was dried by passing through several U-tubes containing freshly roasted quicklime. It was finally liquified in a container surrounded by a mixture of solid carbon dioxide and absolute alcohol. Thus any permanent gases carried over were not collected, and a pure specimen of liquid ammonia was obtained.

V. Description of Apparatus.

The apparatus used for the viscosity experiment is shown in Fig. 2. The gas enters by the tap T_1, and is liquified as previously stated in the bulb A. This was continued until about 6 ccs. of liquid ammonia were condensed. The pressure gauge used was the same as in the first part of the experiment, but now transferred to the entry end of the capillary tube. B is a trap used to prevent the mercury being drawn into the apparatus consequent upon any sudden change of pressure. The small manometer M_1 was used to verify that the exit pressure was atmospheric, which was found to be always the case. The capillary C_1 and tap T_2 served as a means of adjusting the pressure difference. T_3 is a two-way tap connecting to the collecting apparatus R and to the atmosphere. The receiving vessel R was open to the atmosphere through the tube L, which contained glass wool. To prevent temperature changes once steady conditions had been established, the whole apparatus was lagged with cotton wool. For the readings at room temperature the same method for determining the capillary temperature was used as that in calibrating the tube. To obtain the higher temperatures, steam and aniline vapour were used, a distilling apparatus not shown in the figure being set up. It was found that by covering the end corks with tinfoil they resisted the action of the aniline vapour and did not char appreciably. The appropriate liquid (water or aniline) was boiled

sufficiently rapidly to produce a free flow of vapour from the exit end, thus ensuring a uniform temperature inside the jacket equal to the boiling point. The jacket was sloped somewhat towards the exit end to allow condensed vapour to drain away. The boiling points of the vapours for the various atmospheric pressures were found from boiling point tables, Kahlbaum's* values being used.

VI. The Viscosity Experiment.

When enough liquid ammonia was obtained the tap T_1 was closed and the generating apparatus removed. The condensing Dewar D was then lowered until sufficient gas had evaporated to sweep all air from the apparatus, the gauge being lowered to clear its tubes and then raised to send a rapid current of ammonia through the capillaries C and C_1. The bulb A was then surrounded by an empty Dewar flask and the whole heavily wrapped in cotton wool. It was found that, though a suitable driving pressure was soon created, it was not steady enough to allow accurate measurement. This was due to variations in the small quantity of heat reaching the bulb caused by convection currents, etc. This difficulty was overcome by

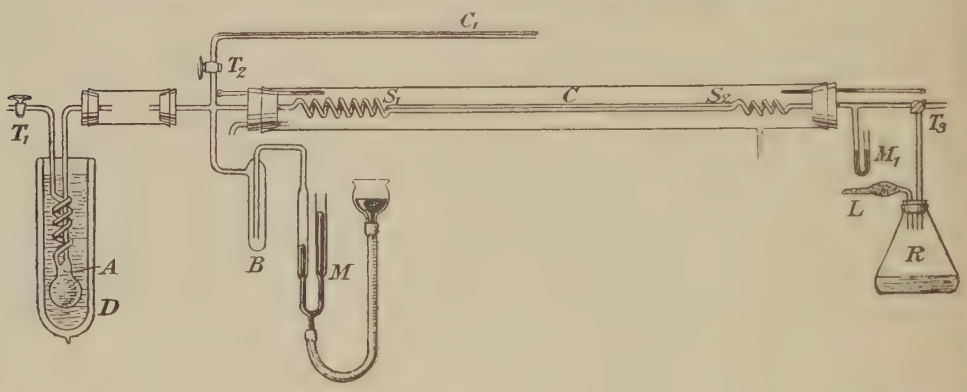


Fig. 2.

introducing a " dead space " equal to four or five times the volume of gas transmitted during a reading, which acted a sort of " buffer " and gave the required steadiness.

Now the critical velocity of transmission for any gas depends on its kinematic viscosity—namely, η/ρ. For ammonia and air this kinematic viscosity is approximately the same. Hence, since the critical velocity for air is given by a pressure difference of 6 cms. of mercury (from first part of the experiment), the pressure difference to be used for ammonia could not exceed this value. The tap T_2 and tube C_1 were used to keep within this range.

Under these conditions the pressure difference remained steady, the greatest variations being usually less than 2·0 per cent. The initial and final portions of the gas were neglected as possibly containing impurities.

The gas, after transpiration, was absorbed in a known volume of standard acid in the receiver R. To test whether the gas was completely absorbed, a moistened litmus paper was put over the exit of L. This being always unaffected showed that all the ammonia was absorbed. At the end of the " run " the excess acid was

* Z. Phs. Chem., 26 (1898).

titrated with standard Na_2CO_3 solution, methyl orange being used as an indicator. The basis of standardisation of the solutions used was taken to be $N/_{10}HCl$, prepared from a constant boiling mixture of that acid.*

VII. Calculation of Results.

The transpiration formula in this case gives

$$\eta_t = K_t \frac{(P_1 - P_2)(P_1 + P_2)}{P_2 V_2} = K_t \frac{\delta(2P_2 + \delta)}{P_2 V_2}$$

where P_2=atmospheric pressure=$P_1 - \delta$.

As before, P_2 and δ are converted to cms. of mercury at 0°C. The appropriate value of the constant is obtained from the experimental value, using the relation

$$K_t = K_0(1 + 3\gamma t)$$

where γ=coefficient of linear expansion of glass=0·000009.

The volume of gas (V_2) at pressure P_2 and the temperature of the jacket is calculated from the weight of gas deduced from the titration results.

Since, however, ammonia does not, especially at atmospheric temperature, behave as a perfect gas, we cannot use the perfect gas equation to determine V_2 from the mass of gas titrated without applying a correction.

Consequently the experimental densities of the gas at various temperatures given by Perman and Davies† were used to obtain this correction.

The values used were—

Temperature.	Litres per gram observed.	Litres per gram PV equation.	Correction.
0°C.	1·2973	1·3161	1·43%
50°C.	1·5473	1·5571	0·64%
100°C.	1·7964	1·7980	0·10%

From this table, by graphing temperatures against correction, the necessary correction at any temperature was found.

The values of the correction thus determined were :—

Temperature.	Correction.
15°C.	1·20 per cent.
100°C.	0·10 per cent.
184°C.	0·0 per cent.

Finally it was necessary to correct the values of the viscosity thus obtained for slipping exactly as in the determination of K.

The correction factors by which the viscosity has to be multiplied are as follows :—

At	15°C.	1·0036
	100°C.	1·0057
	184°C.	1·0073

* cf. Halett and Bonner, J. Am. Chem. Soc., 31, pp. 390-393 (1909).

† Proc. Roy. Soc., Vol. 78., p. 34 (1906).

VIII. Tables of Results.

Table II gives a specimen series of observations.

Table II.

Time.	Temp. of gas.	Barometer.	Manometer A (cms. of Hg) B.		Volume of gas.	Viscosity.
15 mins.	183·6°C.	75·42 cms.	3·97	5·84	23·20 c.cms.	$1.1{\cdot}7\times10^{-6}$
10 ,,	183·8°C.	75·74 ,,	1·37	5·65	35·95 ,,	161·7 ,,
9 ,,	183·8°C.	75·72 ,,	1·66	5·85	31·11 ,,	161·1 ,,
9 ,,	183·8°C.	75·70 ,,	1·72	5·93	32·06 ,,	160·5 ,,
10 ,,	183·95°C.	76·06 ,,	3·935	6·07	17·25 ,,	160·8 ,,
10 ,,	183·9°C.	76·03 ,,	4·79	6·21	11·75 ,,	161·3 ,,

Table III.

t°C.	$\eta\times10^6$	t C.	$\eta\times10^6$	t°C.	$\eta\times10^6$
15°C.	98·9	100·1°C.	131·0	183·8°C.	161·7
	101·7		130·6		161·7
	101·3		127·7		161·1
	101·8		130·2		160·5
	99·5		127·4		160·8
	100·2		130·2		161·3
	101·5		130·8		
	101·8		128·2		
	97·9				
	100·2				

Mean values :

$\eta_{15}=100{\cdot}5\times10^{-6}$: $\eta_{100{\cdot}1}=129{\cdot}5\times10^{-6}$: $\eta_{183{\cdot}8}=161{\cdot}2\times10^{-6}$ C.G.S. units.

The readings at room temperature were corrected to 15°C. by the assumption of a linear rate of change of viscosity with temperature using in conjunction with our own results the mean of Vogel's* and Thomsen's† values at 0°C., viz.: $\eta_0=94{\cdot}3\times10^{-6}$ C.G.S. units (their values were $92{\cdot}6\times10^{-6}$ and $96{\cdot}0\times10^{-6}$ C.G.S. units respectively). For the other two determinations, during which the variations were much smaller, it was sufficient to take the mean temperature for the value.

IX. Calculation of Sutherland's Constant. Discussion of Results.

The accepted formula for change of viscosity with temperature is

$$\eta=\frac{KT^{3/2}}{C+T}$$

where C=Sutherland's constant, T=absolute temperature, K=a constant.

Assuming this relation holds, then $KT^{3/2}/\eta=C+T$. Therefore, $T^{3/2}/\eta$ is a linear function of T. The effect of plotting this is shown in Fig. 3. The accuracy of the

* Berlin Dissertation (1914).

† Ann. der Physik., Vol. 36, p. 825.

experiments in relation to the temperature range is not such as to enable Sutherland's law to be tested; but, assuming it to be true, we may make an estimate of the constant by drawing the best straight line through the points on the graph. The value for C thus obtained is 377, but this must be regarded as subject to considerable error, as the points only fit the best straight line with an accuracy of one per cent.

By extrapolation from the graph, η_0 is found to be equal to $94{\cdot}4\times10^{-6}$ C.G.S.

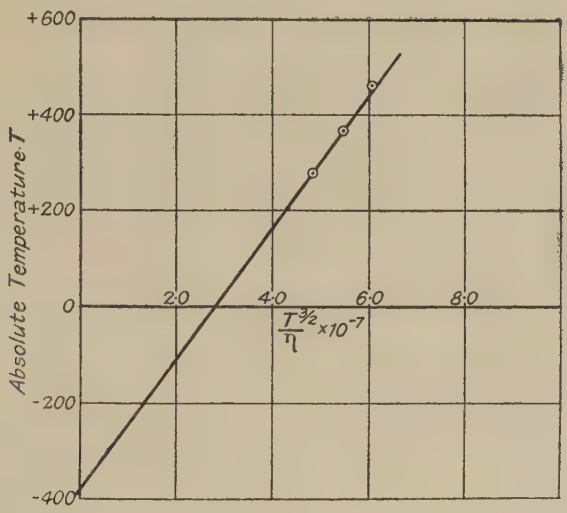


FIG. 3.

units, which compares well with the mean of Vogel's and Thomsen's results quoted above. Prof. Rankine and C. J. Smith* found $\eta_{100}=130{\cdot}3\times10^{-6}$ C.G.S. units, which also agrees well with our value of $129{\cdot}5\times10^{-6}$ C.G.S. units; further, assuming $\eta_0=94{\cdot}3\times10^{-6}$ C.G.S. units, they calculated Sutherland's constant as 370, and, using this result, found the mean collision area ($\bar{A}$) of the ammonia molecule to be $\bar{A}=0{\cdot}640\times10^{-15}$ cms.^{2}. From our data $\bar{A}=0{\cdot}633\times10^{-15}$ cms.^{2}.

DISCUSSION.

Prof. A. O. RANKINE said that in published tables of physical constants the values given for different temperatures have often been obtained by different methods and cannot, therefore, be safely used for studying the variation of a given property with temperature. Hence it is important to apply any given method over as wide a range of temperatures as possible. The authors have covered a range of about 170°C., which is very satisfactory.

Mr. F. E. SMITH said that the authors' work is a valuable indication that the transpiration method is a simple and reliable one for determining viscosities.

* Phil. Mag. 42 (1921)

IV.—VALVE MAINTAINED TUNING FORKS WITHOUT CONDENSERS.

By T. G. Hodgkinson, *A.M.I.E.E.*

Received September 4, 1925.

Abstract.

The valve maintained tuning fork is now a much used piece of apparatus, but the influence of the conductance of the valve electrodes on the design has not been considered. The present paper discusses the design of valve maintained tuning forks without condensers and shows that the conductance of the valve grid decides the direction in which the electrode coils must be wound; and also that it is advantageous, particularly in the case of low frequency forks, to interpose transformers between the valve electrodes and the fork magnets.

THE valve controlled tuning fork was originated by Eccles, and demonstrated before the Physical Society.*

The method was investigated by Butterworth,† who dealt with "untuned" and "tuned" systems, and evaluated the very small differences between the note frequency of the maintained system and the *true fork note.*

The work following employs Butterworth's methods and notation to some extent to amplify the case of the "untuned" fork system, and to clear away ambiguity with regard to the direction of winding of the electrode coils; also to deal with a system employing transformers between the valve electrodes and the fork magnet coils.

The transformer system has obvious advantages for low-frequency forks in that, apart from the additional winding space and improved space factor it provides, it is possible to adjust the phase of the driving current in a way to reduce the difference between the note of the maintained system and the *true fork note* to zero, and in this respect is similar to condenser tuned systems.

Notation.

Mass of fork supposed collected at end of prong	a
Frictional force per unit velocity	β
Control force per unit displacement	γ
Force in magnet gap per unit current in anode coil	A_1
Force in magnet gap per unit current in grid coil	A_2
Plate coil resistance and inductance	r_1 L_1
Grid coil resistance and inductance	r_2 L_2
Plate and grid conductances	s_1 s_2
Plate and grid currents	i_1 i_2

The fork is controlled by the electrode coils, which are wound on pole pieces of a magnet system and arranged close to the prongs of the fork, as shown in Fig. 1.

The effect of the iron losses adds a determinate amount to the effective resistance of the coils, and this amount is supposed included in the resistances r_1 and r_2. The effective inductances of the coils are also reduced by a determinate amount

* Proc. Phys. Soc., Vol. 31, p. 260 (1919).
† Proc. Phys. Soc., Vol. 32, p. 345 (1920).

by the iron losses, and the symbols L_1 and L_2 are intended to indicate the reduced values.

D is the time operator $\delta/\delta t$, and the equation of motion of the fork system is

$$(aD^2+\beta D+\gamma)y=A_1i_1+A_2i_2,$$

where y is the displacement of the mass a.

The currents are supposed counter clockwise, and the coils are similarly wound—i.e., connected in such a way that a current passing from the grid to the plate would maintain flux in the same direction in the magnet system.

The back E.M.F. in the anode coil due to movement of the fork prong is

$$A_1Dy=\frac{A_1^2Di_1+A_1A_2Di_2}{aD^2+\beta D+\gamma}$$

and, similarly, the back E.M.F. in the grid coil is

$$A_2Dy=\frac{A_1A_2Di_1+A_2^2Di_2}{aD^2+\beta D+\gamma}$$

This notation is Butterworth's, and the equation resulting from a systematic

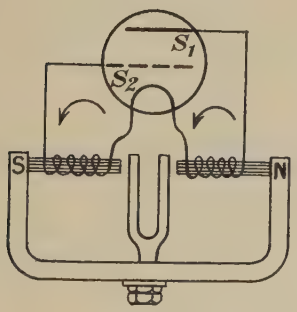


FIG. 1.—FORK SYSTEM WITH MAGNET COILS DIRECTLY CONNECTED TO VALVE ELECTRODES.

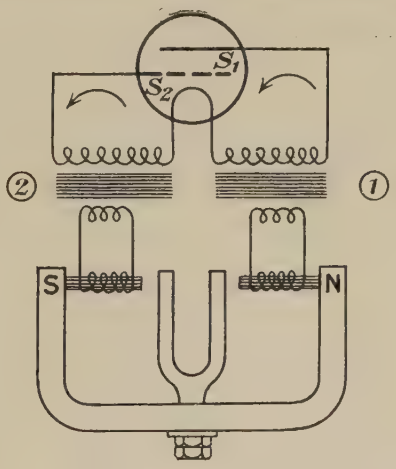


FIG. 2.—FORK SYSTEM WITH TRANSFORMERS BETWEEN THE VALVE ELECTRODES AND FORK MAGNET COILS.

solution of the circuit, which is given below, differs from his in the sign of the valve magnification factor term, and in that it includes a discussion of the effect of the valve conductances.

The circuit equation is

$$\Big[(1/s_1+r_1+L_1D)(1/s_2+r_2+L_2D)(aD^2+\beta D+\gamma)+$$
$$(1/s_1+r_1+L_1D)A_2^2D+(1/s_2+r_2+L_2D)A_1^2D-\frac{m}{s_2}A_1A_2D\Big]i_1i_2=0 \quad (1)$$

m is the valve magnification factor, or $\delta i_1/\delta V_g \div \delta i_1/\delta V_p$, where V_g and V_p are grid and plate volts respectively.

We are only concerned with discussing this equation with regard to our own particular problem, remembering that the decay factor of the fork should be a very small quantity. The force in a magnet gap per unit current A is something like a linear function of the number of turns on the coil, and A_1^2 is conveniently written X_1L_1. In the same way, $A_2^2=X_2L_2$, and if we write $W_F^2=\gamma/a$, $-W^2=D^2$,

$$\frac{L_1s_1W_F}{1+r_1s_1}=b_1 \text{ and } \frac{L_2s_2W_F}{1+r_2s_2}=b_2$$

we can separate the equation (1) into frequency and condition equations in the popular manner to make

Frequency Equation.

$$\left(1-b_1b_2\frac{W^2}{W_F^2}\right)\left(1-\frac{W^2}{W_F^2}\right)-\frac{\beta}{aW_F}\left(b_1+b_2+\frac{b_1b_2}{\beta W_F}\overline{X_1+X_2}\right)\frac{W^2}{W_F^2}=0 \quad . \quad . \quad . \quad . \quad (2)$$

Condition Equation.

$$(b_1+b_2)\left(1-\frac{W^2}{W_F^2}\right)+\frac{\beta}{aW_F}\left(1-b_1b_2\frac{W^2}{W_F^2}+\frac{X_1b_1+X_2b_2}{\beta W_F}-\frac{m}{1+r_2s_2}\cdot\frac{A_2}{A_1}\frac{b_1X_1}{\beta W_F}\right)\gtrless 0 \quad (3)$$

The two signs of the condition equation $\gtrless 0$ depend on the solution of the frequency equation selected for substitution therein—i.e., if the solution of the frequency equation is below the true fork note, the upper sign is required, and vice versa.

Ignoring small quantities, the useful solution of the frequency equation is

$$1-\frac{W^2}{W_F^2}=\frac{\beta/aW_F}{1-b_1b_2}\left(b_1+b_2+\frac{b_1b_2}{\beta W_F}\overline{X_1+X_2}\right) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (4)$$

β/aW_F need only be a small quantity like 0·001 ; b_1 will be made approximately unity in order to use the valve effectively, and b_2 can be made small by working on a negative grid. The values of $X_1/\beta W_F$ and $X_2/\beta W_F$ will transpire subsequently, but will be something like, or less than, unity.

Substituting from equation (4), the *Condition Equation* is

$$(1+b_1^2)(1+b_2^2)-\frac{1}{\beta W_F}\left\{\frac{m}{1+r_2s_2}\frac{A_2}{A_1}(1-b_1b_2)X_1b_1-(1+b_2^2)X_1b_1-(1+b_1^2)X_2b_2\right\}$$

$$\gtrless 0 \quad . \quad . \quad (5)$$

If the product b_1b_2 is appreciably less than unity, the frequency solution gives a note slightly lower (the difference is very small) than the true fork note, and if $X_2=X_1$

$$\frac{X_1}{\beta W_F}>\frac{(1+b_1^2)(1+b_2^2)}{\frac{m}{1+r_2s_2}(1-b_1b_2)b_1-(1+b_2^2)b_1-(1+b_1^2)b_2} \quad . \quad . \quad . \quad . \quad . \quad (6)$$

The coils require to be similarly wound—i.e., $A_2=A_1$—and this case includes the non-conducting grid system as a special case.

If the grid conductance is negligible

$$1-W^2/W_F^2=\frac{\beta}{\alpha W_F}b_1 \text{ and } \frac{X_1}{\beta W_F}\rightarrow\frac{1+b_1^2}{b_1 m-1}$$

$\delta/\delta b_1 \frac{X_1}{\beta W_F}$ is negative until $b_1=1$, so that the minimum value of $\frac{X_1}{\beta W_F}\rightarrow\frac{2}{m-1}$

Guess m is 7 and $\frac{X}{\beta W_F}\rightarrow 1/3$.

This is the ideal valve system, of course, and a value for the grid conductance increases $X/\beta W_F$ appreciably.

In a special case, say $b_1=b_2$,

$$\frac{X_1}{\beta W_F}\rightarrow\frac{(1+b_1^2)^2}{b_1\left[\frac{m}{1+r_2 s_2}(1-b_1^2)-2(1+b_1^2)\right]} \quad \ldots\ldots (7)$$

Guess $\frac{m}{1+r_2 s_2}=6$, and the best value for b_1 works out about 1/3, and $X_1/\beta W_F\rightarrow 1{\cdot}19$.

Working on positive grids and with large inductances so that the product $b_1 b_2$ is appreciably greater than unity, the note of the maintained system is slightly higher than the true fork note.

$$1-W^2/W_F^2=-\frac{\beta/\alpha W_F}{b_1 b_2-1}\left(b_1+b_2+\frac{b_1 b_2}{\beta W_F}\overline{X_1+X_2}\right) \quad \ldots\ldots (8)$$

The coils now require to be oppositely wound, and if $X_2=X_1$ and $A_2=-A_1$

$$\frac{X_1}{\beta W_F}\rightarrow\frac{(1+b_1^2)(1+b_2^2)}{\frac{m}{1-r_2 s_2}(b_1 b_2-1)b_1-(1+b_2^2)b_1-(1+b_1^2)b_2} \quad \ldots\ldots (9)$$

Guess $\frac{m}{1+r_2 s_2}=5$ and $b_1=b_2$, the best value of b_1 works out about 3·125 and $\frac{X_1}{\beta W_F}\rightarrow 1{\cdot}66$.

The guesses at $\frac{m}{1+r_2 s_2}$ probably favour the latter case to some extent, but the general results are borne out by experiment; indeed, fork systems can be designed so that when the valve filament is relatively dull and the grid is negative the fork is maintained with the coils *similarly* wound, while, if the valve filament is bright and the grid is positive, the fork is only maintained when the grid coils are *oppositely* wound.

The special case when the product $b_1 b_2$=unity is not a profitable one. The frequency solution is $1-W^2/W_F^2=\pm\sqrt{\frac{\beta}{\alpha W_F}}$, and the system is correspondingly difficult to support in vibration.

The decay factor of a higher frequency fork is difficult to measure directly, but can be determined by the method advocated by A. Campbell,* using a vibration galvanometer connected to a coil, the induction of which is varied by the movement of the fork.

This method was investigated analytically by R. Ll. Jones.†

Alternatively, the quantity A^2/β can be measured as a part of the effective resistance of the magnet coil tested on a bridge at the resonant frequency of the fork; but this measurement requires great care and careful measurement, on the same bridge, of the effective copper and iron resistances with the fork held still. With lower frequency forks the decay factor can be determined with a microscope and stop watch.

If the iron losses for a magnet coil may be represented as due to a circuit completely linked with the coil and having resistance r_I and inductance l_I, then the part of the effective resistance of the coil due to iron losses is $\frac{LWI}{1+I^2}$ and the effective inductance is $\frac{L}{1+I^2}$, where L is the inductance of the magnet coil and I is $\frac{l_I W}{r_I}$

I can be predetermined for a specific case in terms of the hysteresis coefficient, permeability, flux densities and Joulean resistance, and may be as much as 0·2, but should be less.

The importance of keeping the losses small is apparent by their effect in reducing $b=\frac{LsW}{1+rs}$ and $\frac{m}{1+r_2 s_2}$

$A^2/L=X$ will have something of the form

$$X=\frac{B_0^2}{4\pi R_g}\cdot\frac{R_a}{R_a+R_g}$$

where B_0 is the flux density in the gap, R_a is the leakage reluctance, R_g is the gap and iron circuit reluctance.

The importance of keeping the leakage reluctance large is obvious.

The 1,000 ~ fork made by the Cambridge Scientific Instrument Co. has the requirements of design for an "untuned" system working on a negative grid, and although this fork is provided with condensers it is maintained in oscillation without them.

Transformer Coupled System.

The dimensions of the higher frequency "commercial" forks lend themselves to the design of valve control systems, but low frequency forks are more difficult.

The principal difficulty is that of making the impedance of the anode magnet coil compete with the anode resistance without making the copper and iron losses impossibly large and the leakage reluctance too small. Again, the decay factors of these forks are frequently greater than those of higher frequency forks.

The impedance difficulty can be overcome by interposing suitable transformers between the valve electrodes and the magnet coils. In addition to the advantages

* Proc. Phys. Soc., Vol. 31, p. 87 (1919).
† Proc. Phys. Soc., Vol. 34, p. 67 (1922).

enumerated earlier of additional and improved space factors and capability of adjusting the difference between the note of the system and the true fork note, the steady flux may be provided by direct current in the magnet coils, and so conveniently adjusted for experimental purposes.

Notation.

Resistance and inductance of valve winding of transformer (1) ... r_1 l_1
Resistance and inductance of valve winding of transformer (2) ... r_2 l_2
Resistance and inductance of fork winding of transformer (1) ... ρ_1 λ_1
Resistance and inductance of fork winding of transformer (2) ... ρ_2 λ_2
Mutual coupling transformer (1) ... ... M_1
Mutual coupling transformer (2) ... ... M_2
Resistance and inductance of fork magnet coil (1) ... ... P_1 L_1
Resistance and inductance of fork magnet coil (2) ... ... P_2 L_2

In what follows the transformers have been dealt with as without leakage, although this does not affect the conclusion drawn if the leakage is small and is regarded as included in the fork magnet coil inductances.

A systematic solution of the circuits gives the differential equation

$$\Big[\{(1+s_1\overline{r_1+l_1D})(P_1+\rho_1+\overline{L_1+\lambda_1D})-s_1M_1{}^2D^2\}\{(1+s_2\overline{r_2+l_2D})(P_2+\rho_2+\overline{L_2+\lambda_2D})-s_2M_2{}^2D^2\}$$
$$+\{(1+s_1r_1+l_1D)(P_1+\rho_1+L_1+\lambda_1D)-s_1M_1{}^2D^2\}(1+s_2\overline{r_2+l_2D})\frac{A_2{}^2D}{aD^2+\beta D+\gamma}$$
$$+\{(1+s_2\overline{r_2+l_1D})(P_2+\rho_2+\overline{L_2+\lambda_2D})-s_2M_2{}^2D^2\}(1+s_1\overline{r_1+l_1D})\frac{A_1{}^2D}{aD^2+\beta D+\gamma}$$
$$-s_1M_1M_2D^2\frac{mA_1A_2D}{aD^2+\beta D+\gamma}\Big]\,i_1i_2=0 \quad \ldots\ldots\ldots\ldots\ldots \quad (10)$$

Those who have read Butterworth's Paper referred to, or who have analysed fork circuits, will recognise this equation as similar in form to that of a fork circuit with condensers across the electrodes ; and, indeed, the transformer coupled system behaves like a condenser tuned system, in that it is capable of reducing the difference between the note maintained by the system and the true fork note.

Adding to the notation

$$\frac{P_1+\rho_1}{L_1W_F}=a_1 \qquad \frac{L_1+\lambda_1}{L_1}=c_1 \qquad \frac{l_1s_1W_F}{1+r_1s_1}=b_1$$
$$\frac{P_2+\rho_2}{L_2W_F}=a_2 \qquad \frac{L_2+\lambda_2}{L_2}=c_2 \qquad \frac{l_2s_2W_F}{1+r_2s_2}=b_2$$

and separating the phases.

The *Frequency Equation* becomes

$$\Big(1-\frac{W^2}{W_F{}^2}\Big)\Big[\Big(a_1-b_1\frac{W^2}{W_F{}^2}\Big)\Big(a_2-b_1\frac{W^2}{W_F{}^2}\Big)-(c_1+a_1b_1)(c_2+a_2b_2)\frac{W^2}{W_F{}^2}\Big]$$
$$-\frac{\beta}{\alpha W_F{}^2}\frac{W^2}{W_F{}^2}\left[\begin{array}{l}\Big(a_1-b_1\dfrac{W^2}{W_F{}^2}\Big)(c_2+a_2b_2)+\dfrac{X_2}{\beta W_F}\Big(c_1+a_1b_1+b_2\overline{a_1-b_1\dfrac{W^2}{W_F{}^2}}\Big)+\\ \Big(a_2-b_2\dfrac{W^2}{W_F{}^2}\Big)(c_1+a_1b_1)+\dfrac{X_1}{\beta W_F}\Big(c_2+a_2b_2+b_1\overline{a_2-b_2\dfrac{W^2}{W_F{}^2}}\Big)\end{array}\right]=0 \quad (11)$$

and *Condition Equation*

$$\left(1-\frac{W^2}{W_F{}^2}\right)\left[\left(a_1-b_1\frac{W^2}{W_F{}^2}\right)(c_2+a_2b_2)+\left(a_2-b_2\frac{W^2}{W_F{}^2}\right)(c_1+a_1b_1)\right]+$$

$$\beta/\alpha W_F\left[\begin{array}{l}\left(a_1-b_1\dfrac{W^2}{W_F{}^2}\right)(a_2-b_2W^2/W_F{}^2)-(c_1+a_1b_1)(c_2+a_2b_2)\dfrac{W^2}{W_F{}^2}+\\ \dfrac{X^2}{\beta W_F}\left\{a_1-b_1\dfrac{W^2}{W_F{}^2}-b_2\dfrac{W^2}{W_F{}^2}(c_1+a_1b_1)\right\}+\\ \dfrac{X_1}{\beta W_F}\left\{a_1-b_1\dfrac{W^2}{W_F{}^2}-b_1\dfrac{W^2}{W_F{}^2}(c_2+a_2b_2)+\dfrac{mb_1}{1+r_2s_2}(c_1-1)\dfrac{W^2}{W_F{}^2}\cdot\dfrac{M_2A_2L_1}{M_1A_1L_2}\right\}\end{array}\right]=0 \quad (12)$$

There are too many quantities in these equations to be discussed independently, but if we make the transformers of the two electrodes similar and the magnet coils similar, $a_1=a_2$ and $c_1=c_2$, but b_1 and b_2 are dependent on the valve electrodes.

Write $\dfrac{b_1-a_1}{c_1+a_1b_1}=\varphi_1$ and $\dfrac{b_2-a_2}{c_2+a_2b_2}=\varphi_2$, $X_2=X_1$, and the *Frequency Equation* becomes

$$1-\frac{W^2}{W_F{}^2}=\frac{\beta/\alpha W_F}{1-\varphi_1\varphi_2}\left[\varphi_1+\varphi_2-\frac{2X}{\beta W_F}\cdot\frac{(1-a\varphi_1+\varphi_2-c\varphi_1\varphi_2)}{c+a^2}\right] \quad . \quad . \quad . \quad . \quad (13)$$

and substituting this value in the condition equation the condition equation becomes

$$\frac{X}{\beta W_F}=\frac{(1+\varphi_1{}^2)(1+\varphi_2{}^2)}{\dfrac{c-1}{c+a^2}\left[\pm\dfrac{m}{1+r_2s_2}\dfrac{(c\varphi_1+a)(1-a\varphi_2)(1-\varphi_1\varphi_2)}{c+a^2}-\varphi_1(1+\varphi_2{}^2)-\varphi_2(1+\varphi_1{}^2)\right]-\dfrac{2a}{c+a_2}(1+\varphi_1{}^2)(1+\varphi_2{}^2)} \quad . \quad . \quad (14)$$

When the product $\varphi_1\varphi_2$ is small compared with unity, m requires the positive sign—i.e., the circuits are similarly wound; but when $\varphi_1\varphi_2$ is greater than unity, m requires the negative sign, or the circuits are oppositely wound.

If a is negligibly small, the frequency equation becomes

$$1-\frac{W^2}{W_F{}^2}=\frac{\beta/\alpha W_F}{1-\varphi_1\varphi_2}\left(\varphi_1+\varphi_2+\frac{2X}{\beta W_F}(\varphi_1\varphi_2-1/c)\right). \quad . \quad . \quad . \quad . \quad (15)$$

and the *Condition Equation*

$$\frac{X}{\beta W_F}=\frac{(1+\varphi_1{}^2)(1+\varphi_2{}^2)}{\dfrac{c-1}{c}\left[\pm\dfrac{m\varphi_1}{1+r_2s_2}(1-\varphi_1\varphi_2)-\varphi_1(1+\varphi_2{}^2)-\varphi_2(1+\varphi_1{}^2)\right]} \quad . \quad . \quad . \quad . \quad (16)$$

and whatever has been said of the optimum values of b_1 in the case of the direct coupled system applies to φ in the case of this transformer coupled system.

The optimum value of c in this transformer case is limited only by practical considerations, but a value for a reduces the optimum values of both φ and c very appreciably.

Returning to the general case of equations (13) and (14), notice that if the product $\varphi_1\varphi_2$ is less than unity it is possible to adjust φ and c to make the difference $1-\dfrac{W^2}{W_F{}^2}=0$.

Indeed, if a_1 has value this condition may easily give the lowest value of $X/\beta W_F$ in the condition equation.

The equation connecting φ and c to achieve this result is

$$\frac{m}{1+r_2s_2}\cdot\frac{c-1}{c+a^2}(1-a\varphi_2)(a+c\varphi_1)(\varphi_1+\varphi_2)$$
$$-(1+\varphi_1^2)(1+c\varphi_2^2)-(1+\varphi_2^2)(1+c\varphi_1^2)=0 \quad \ldots\ldots (17)$$

a is a function of c for any particular frequency and any particular space factor so that this equation becomes a quadratic in c.

If the grid conductance is entirely negligible, $b_2=0$ and $\varphi_2=-a/c$, and the *Frequency Equation* (13) reduces to

$$1-\frac{W^2}{W_F^2}=\frac{\beta/aW_F}{1+a\varphi_1/c}\left(\varphi_1-a/c-2/c\,\frac{X}{\beta W_F}\right) \quad \ldots\ldots (18)$$

While the *Condition Equation* (14) becomes

$$\frac{X}{\beta W_F}>\frac{1}{\frac{c-1}{c+a^2}\frac{}{m-1}\left(\frac{a/c}{1+a^2/c^2}+\frac{\varphi_1}{1+\varphi_1^2}\right)-\frac{2a}{c^2+a^2}} \quad \ldots\ldots (19)$$

If $a=fc$ the special condition equation (17) becomes

$$c=\frac{2+\overline{m+1}\,\varphi_1^2-\overline{m-1}f^2}{(m-1-2f^2)\varphi_1^2-\overline{m+1}f^2} \quad \ldots\ldots\ldots (20)$$

The value of φ_1 which gives the smallest value for $X/\beta W_F$ does not give the true fork note. As an example, if $f=0{\cdot}2$ and $m=7$, say $c=4$.

The optimum value of φ_1 is unity.

$$1-W^2/W_F^2=0{\cdot}59\beta/aW_F \text{ and } \frac{X}{\beta W_F}>0{\cdot}39.$$

On the other hand, if $\varphi_1=0{\cdot}44$

$$1-W^2/W_F^2=0 \text{ and } \frac{X}{\beta W_F}>0{\cdot}48.$$

The negative grid gives, of course, the ideal transformer case, and a value for b_2 increases $\frac{X}{\beta W_F}$ very appreciably.

If the grid conductance is equal to the plate conductance $\varphi_2=\varphi_1$, and the Frequency Equation (13) becomes

$$1-\frac{W^2}{W_F^2}=\frac{2\beta/aW_F}{1-\varphi^2}\left(\varphi-\frac{X}{\beta W_F}\cdot\frac{(1-2a\varphi-c\varphi^2)}{c+a^2}\right) \quad \ldots\ldots (21)$$

and the Condition Equation (14) becomes

$$\frac{X}{\beta W_F}>\frac{1}{\frac{c-1}{c+a^2}\left\{\frac{m}{1+r_2s_2}\cdot\frac{(1-a\varphi)(1-\varphi^2)(a+c\varphi)}{(1+\varphi^2)^2(c+a^2)}-\frac{2\varphi}{1+\varphi^2}\right\}-\frac{2a}{c+a^2}} \quad \ldots\ldots (22)$$

As an example, say $\frac{m}{1+r_2s_2}=6$ and $f=0{\cdot}2$. The optimum value of c is in the neighbourhood of 4·4, and the optimum value of $\varphi=0{\cdot}13$.

$$1-\frac{W^2}{W_F{}^2}=0{\cdot}59\beta/\alpha W_F \quad \text{and} \quad \frac{X}{\beta W_F}=2{\cdot}35.$$

The value of φ giving the true fork note is 0·235, and under these conditions $\frac{X}{\beta W_F}=2{\cdot}87$.

When the product $\varphi_1\varphi_2>1$, m requires the negative sign (i.e., the circuits require to be oppositely wound), the values of $X/\beta W_F$ increase very rapidly, and there is no opportunity of equating the note of the system to the true fork note.

As an example, say $f=0{\cdot}1$ and $\varphi_1=\varphi_2=2$; the optimum value of c is in the neighbourhood of 1·4, and the system would not work on the valve we have used for the other systems, with $m=7$. If, however, $\frac{m}{1+r_2s_2}=12$, or double the magnification factor of the valve used previously

$$1-\frac{W^2}{W_F{}^2}=-12\beta/\alpha W_F \quad \text{and} \quad \frac{X}{\beta W_F}=5{\cdot}46.$$

If $\beta/\alpha W_F$ is large, this would not be a case we could deal with in this approximate way; but the case is not a profitable one in any sense.

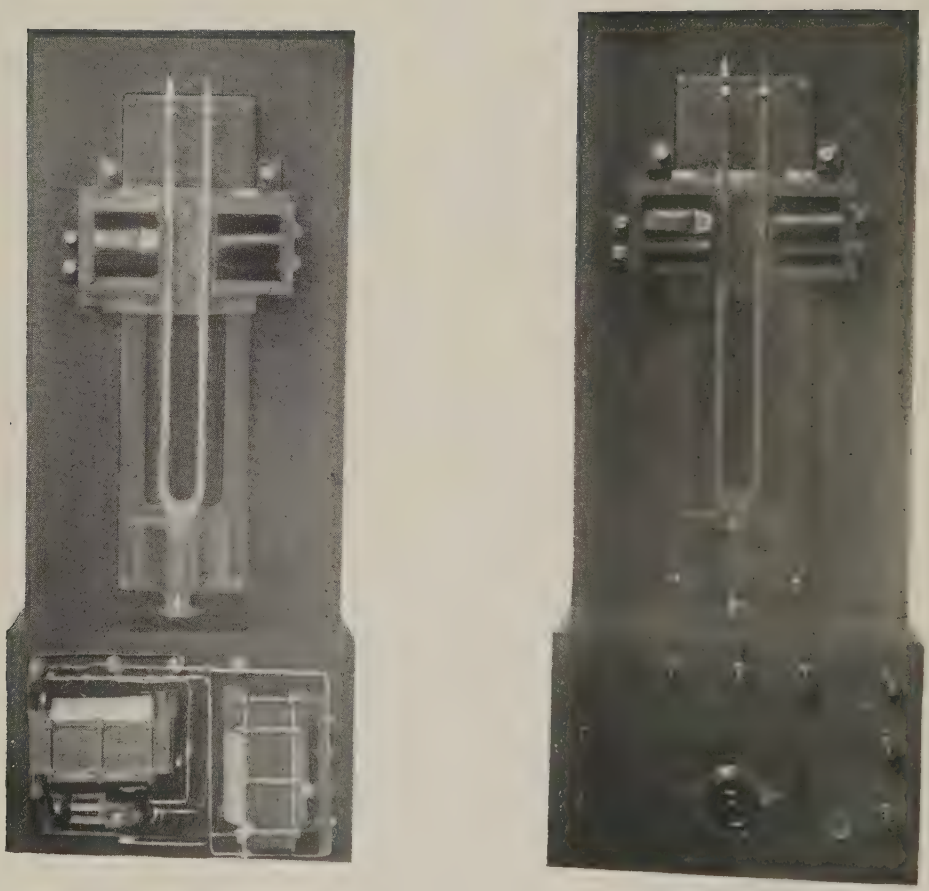
In the foregoing work an effort has been made to determine the best system of design for low frequency forks. It requires no analysis to discover that losses should be kept small or that systems working on negative grids are better than those working on conducting grids; however, it is useful to know to what extent these losses may be incurred. If the grid is sufficiently negative to prevent appreciable rectification, the grid conductance need not concern the design at all. On the other hand, a special grid battery is an unnecessary expense, and a rectifying grid is easily dealt with in design. Indeed, it is not until positive volts are put on grids that grid conductances require special consideration, unless coil resistances are too large and the product r_2s_2 has a significant value.

A design for a *direct coupled* system working on a reception valve with grid conductance (say about one-fifth the plate conductance) would make $\frac{m}{1+r_2s_2}=6$ and $\frac{l_1s_1W_F}{1+r_1s_1}=0{\cdot}7$.

This would give with similar magnets $\frac{X}{\beta W_F}=0{\cdot}51$ and $1-W^2/W_F{}^2=1{\cdot}05\,\beta/\alpha W_F$.

With low frequency forks $\frac{l_1s_1W_F}{1+r_1s_1}=0{\cdot}7$ may be a difficult achievement for a *direct coupled* system, but easy with a closed iron circuit transformer between electrodes and magnets.

The improved space factor of the magnet coils using heavier wire together with the added space of the transformer makes small values of $a=fc=\frac{P+\rho}{LW_F}$ possible.



PHOTOGRAPH OF FORK SHOWING TRANSFORMERS.

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If a battery is used to provide the steady flux, the magnet wire can be selected for small voltages.

A design for a transformer coupled system is embodied in the quantities

$$c=4 \quad \text{or} \quad \lambda/L=3$$
$$a=1 \quad \text{or} \quad f=0{\cdot}25$$
$$\frac{m}{1+r_2 s_2}=6, \quad \varphi_1=0{\cdot}35, \quad \varphi_2=-0{\cdot}05.$$

This, with similar magnets on both electrodes, would make

$$\frac{X}{\beta W_F}=0{\cdot}97 \quad \text{and} \quad 1-W^2/W_F{}^2 \text{ negligible.}$$

An actual example of a 50 cycle fork design is given below.

Similar Transformers.

Electrode winding	14,000 turns	40	S.W.G. copper.
Magnet circuit	500 ,,	22	,, ,,
Magnet coils	1,000 ,,	22	,, ,,

$l=144$ henries	$r=4{,}300$ ohms including iron losses
$\lambda=0{\cdot}23$,,	$\rho=4{\cdot}4$,, ,, ,,
$L=0{\cdot}17$,,	$P=6{\cdot}2$,, ,, ,,

This makes $c=2{\cdot}35$, $a=0{\cdot}2$, $\varphi_1=0{\cdot}55$

The steady flux is provided by a 2 volt battery, and the fork works well on negative or rectifying grids, with any plate voltage above 20 volts.

The fork prongs are 31·5 cm. long, 0·6 cm. deep by 1·5 cm. wide, the magnet coils are wound on E-shaped stalloy stampings with an effective area of 2 square cms.

β/aW_F measured with a microscope and stop watch is 0·002, and the valve is an R7, with a magnification factor about 7·5 and a plate conductance $4{\cdot}4\times10^{-5}$ mhos.

This design is generous in material, but the value of c is roughly right to give the true fork note with a non-conducting grid.

NOTE ADDED NOVEMBER 21, 1925.

The true fork note frequency is 50(1+0·00007). The maintained note, with the valve grid connected to the negative end of the filament battery and for an amplitude of vibration of 0·7 mm. at the ends of the prongs, is 50 (1−0·00012). For an amplitude of 2 mm. it is 50 (1−0·00056). These are comparative measurements only, and have an accuracy ±0·002 per cent. approx. The difference between the true fork note and the 0·7 mm. amplitude maintained note gives a value for $1-W^2/W_F{}^2=0{\cdot}00038$ or 0·19 β/aW_F, a value that would have been impracticable with a direct-coupled system.

I should like to express my indebtedness to Mr. A. Hinchliffe, whose help in the work has been invaluable, and to the Director of Artillery for his permission to offer the Paper for publication.

DISCUSSION.

Dr. W. H. ECCLES said that every one who wishes to use valve maintained forks with precision must read Mr. Butterworth's Paper on the subject, but that Paper does not explain the ambiguity which exists as to the direction of winding of the grid coil. This puzzle has been cleared up by the author, who has also shown how account must be taken of the valve characteristics in designing the apparatus, and how to secure that the maintained frequency shall be

the true frequency of the fork. The latter point is particularly important when the harmonics of the fork frequency are employed: thus it is possible to utilise the twenty-ninth harmonic of a 1,000∞ fork, giving a frequency of 60,000∞. In this way standards can be obtained and oscillations generated over a wide range of the wireless spectrum, at constant and accurately known frequencies.

Dr. C. V. Drysdale said that he had long been interested in stroboscopic methods of measurement, but these have always suffered from the defect that the rate of a fork is varied by the means adopted for maintaining vibration. Dr. Eccles' invention of the valve-maintained fork is of great importance as a solution of this difficulty, and the author is to be congratulated on having put the matter on a satisfactory quantitative basis.

Mr. S. Butterworth (communicated): I have read Mr. Hodgkinson's Paper with great interest. He is quite right when he states that in my Paper on the same subject I gave an erroneous sign to the proper direction of winding for the grid and anode coils in order to maintain the fork in vibration. It may be of interest to point out that the quantities b_1 and b_2 represent the ratio of reactance to resistance in the plate and grid circuits respectively, so that the condition given in the simplest case dealt with by Mr. Hodgkinson (the case of no grid current and no transformers) is that for the greatest ease of maintenance the reactance of the anode circuit should be equal to its resistance. Also, if we remember that the motional impedance of a fork system may be represented by an inductance, capacity and conductance in parallel, the condition that the system will produce sustained vibrations (when the reactance-resistance condition above is satisfied) is that the mutual conductance of the valve shall exceed the anode conductance by an amount at least twice the motional conductance representing the losses in the fork system. The treatment of the case where transformers are used has been very thorough. If we idealise this case, and suppose the transformers perfectly coupled, free from loss, and with fork windings of impedance large compared with the coils driving the fork, then the use of the electrical equivalent of the motional impedance of the fork enables us to show that the transformer system may be replaced by a system free from transformers, in which all the impedances are multiplied by the square of the ratio of transformation. This for equal anode and grid transformers. This is a result something like that arrived at by Mr. Hodgkinson in equation (16). I do not quite understand the particular virtue of making the actual fork frequency exactly equal to the free fork frequency. The special value of these systems lies in their constancy, and, as has been pointed out by Mr. Dye, certain adjustments of the valve voltages may be made to enhance the steadiness. These adjustments are the more readily carried out if the valve system is working well above the limits of maintenance. The reason for supplying condensers to fork systems at 1,000 frequency is partly to enable a greater load to be taken from the system and partly to have greater ease in carrying out the adjustments for the steadiest condition.

Author's reply (communicated): Replying to Prof. Eccles, I have not discussed the use of harmonics, being mainly concerned with producing a low-frequency source in imitation of the noise made by an aeroplane.

The fact that the fork is barely audible even when the fundamental amplitude is very large is evidence of the approximate purity of the note.

The actual difference between the maintained note and the true fork note is not so important as is the constancy of the maintained note and its dependence on such factors as filament emission, gap field, etc. I have included consideration of the valve conductances in the investigation of condenser tuned systems for my own information, and, in general, well-designed condenser tuned systems give smaller differences between the maintained note and the fork note than systems without condensers; however, there is nothing to prevent the tuning of the transformer coupled system, possibly with benefit to the constancy of the note.

I thank Dr. Drysdale for his appreciation of the aim of the Paper, which was to emphasise the importance of the electrode conductances without encroaching too much on ground so thoroughly prospected by Prof. Eccles and Mr. Butterworth.

I wish to thank Mr. Butterworth also for his very interesting communication, and, in reply to his implied question, I think it is an advantage to know that the transformer system is capable of design so that there is only a small difference between the maintained note and the fork note. Although this capability does not necessarily improve the constancy of the note, it does not diminish it, and allows a certain measure of adjustment.

I thoroughly appreciate the effect of condensers in improving the maintenance conditions of tuning forks, and my reference to the Cambridge Co.'s fork was by way of illustration only.

7.—THE TIMES OF "SUDDEN COMMENCEMENTS" (S.C.s) OF MAGNETIC STORMS: OBSERVATION AND THEORY.

By C. CHREE, *Sc.D., LL.D., F.R.S.*

Received October 8, 1925.

ABSTRACT.

An earlier Paper (Proc. Phys. Soc., Vol. 26, 1914, p. 137) discussed a number of data for the times of occurrence of the "Sudden Commencements" (S.C.s) of magnetic storms which had been published by Dr. L. A. Bauer in "Terrestrial Magnetism," and came to a conclusion adverse to Dr. Bauer's claim that S.C.s were propagated from east to west or west to east, with velocities of from 100 to 200 kilometres per second. Since that date Papers on the subject have been published by Prof. S. Chapman and Fr. Rodés, of the Ebro Observatory, propounding theories differing from one another and from Dr. Bauer's. In a recent Paper Dr. Bauer and Mr. W. J. Peters have re-discussed the subject. They conclude that the motion in longitude is much more rapid than according to Dr. Bauer's original estimate, and suggest that is is really a case of propagation from the magnetic equator towards either magnetic pole.

The Section of Terrestrial Magnetism and Electricity of the International Union of Geodesy and Geophysics has recently approved a scheme aiming at the construction and use of special instruments to find out whether S.C.s have a finite rate of propagation. The present Paper discusses the whole subject, partly with a view to facilitating a decision as to the new apparatus and the stations most suitable for the investigation.

§ 1.

THE question whether "sudden commencements" (S.C.s)—the rapid movements which often appear as precursors of magnetic storms—occur simultaneously over the earth, or are propagated with a velocity small compared with that of light, is of much interest to magneticians. It is obviously of fundamental importance in any theory of magnetic storms. The fact that the times of occurrence of an S.C. at different parts of the earth agree to within a few minutes has long been accepted. That the times differ to an extent measurable by ordinary magnetographs seems to have been first suggested by Dr. L. A. Bauer.* From a consideration of a variety of S.C.s recorded prior to 1910, he concluded that magnetic storms do not begin at precisely the same instant all over the earth. To quote his own words, (l.c., p. 232): "The abruptly beginning ones (i.e., S.C.s) . . . appear to progress . . . more often eastwardly, though also at times westwardly, at a speed of about 100 km. to 200 km. per second, so that if a complete circuit of the earth were made it would require, on the average, between seven and three minutes." Mr. R. L. Faris,† of the U.S. Coast and Geodetic Survey, from an investigation of 15 S.C.s, as recorded between 1906 and 1909 at the Survey's five observatories, supported Dr. Bauer's conclusion, obtaining $3\frac{1}{2}$ minutes as the mean time of propagation round the earth. After examining Mr. Faris' data critically, the present writer‡ ventured on the opinion that the differences in the times recorded were most likely due to instrumental or observational causes. Shortly afterwards Dr. Bauer issued an invitation to magnetic observatories to send him the times for the fifteen S.C.s selected by Mr. Faris, as

* "Terrestrial Magnetism," Vol. 15, pp. 9, 111, etc. (1910).
† "Terrestrial Magnetism," Vol. 15, p. 93.
‡ Proc. Phys. Soc., Vol. 23, p. 49.

derived at each observatory from the curves of that observatory. A number of observatories responded, and Dr. Bauer published the data sent him in the June and September issues of "Terrestrial Magnetism" for 1911. The September issue also contained a revised set of data for the Coast and Geodetic Survey's observatories. In explanation of these Dr. Bauer quoted a request he had addressed to the Superintendent of the Survey to "furnish time scalings of all the disturbances . . . taking into consideration the specimen curves from Potsdam and Kew, the tracings of which are enclosed herewith." "I would suggest," he added, "that chief consideration be paid to the Potsdam curves, the precise point of scaling of which has been indicated by the observer (Dr. Venske)."

§ 2.

The data thus published by Dr. Bauer have been discussed, following the historical order, by Prof. G. Angenheister,* myself,† Prof. S. Chapman,‡ and Fr. Rodes§ of Tortosa, and now finally by Dr. Bauer and Mr. W. J. Peters.‖ The two earlier discussions were concerned mainly with the bearing of the data on Dr. Bauer's theory, but I also considered the possibility of propagation north and south. The conclusions reached by Prof. Angenheister and myself were unfavourable to Dr. Bauer's theory. Prof. Chapman also drew conclusions (l.c., p. 210) unfavourable to this theory, but he was mainly concerned in presenting an alternative theory. Supposing magnetic storms to be due to discharges of ions from the sun, he considered what would happen if a stream of ions rotated with the sun in the manner suggested by the 27-day interval in magnetic storms. On this view the S.C. might represent the engulfing of the earth in the ionic stream. As the earth's period round the sun is 365 days, and that of the stream only 27, the stream overtakes the earth, and its advance edge will cross the earth from one end of a diameter to the other in about 30 seconds. On this view there might be an extreme difference of 30 seconds in the times of occurrence of the S.C. on the earth, the propagation being apparently from east to west on one hemisphere and from west to east on the other.

Fr. Rodés, who appeared to be unaware of the previous Papers by Angenheister, Chapman and myself, refers first to several more recent S.C.s, the times of occurrence of which at Tortosa he had compared with times supplied him for other stations. In some cases he mentioned "It would seem probable that . . . the results would indicate a simultaneous beginning all over the earth." "There are, nevertheless," he added, "some cases in which a propagation is strongly suspected" (l.c., p. 162). To account for these he advanced a theory which agrees with Prof. Chapman's in supposing the S.C. due to the immersion of the earth in a cloud of electric particles. But he did not think it possible "that a cloud at a distance of 150 million kilometers in free space has an effective transversal velocity greater than 2 kilometers per second caused by the solar rotation." He adds: "A rough approximation of the time required for the earth to become involved in the cloud can be easily obtained from its orbital velocity; this amounts to about six and a half minutes. . . . . It may be that when half, or even a greater part of the earth, is immersed

* "Nachrichten der K. Gesell. der Wiss. zu Göttingen Math. Phys. Klasse" (1913).
† Proc. Phys. Soc., Vol. 26, p. 137.
‡ Proc. Phys. Soc., Vol. 30, p. 205.
§ "Terrestrial Magnetism," Vol. 27, p. 161.
‖ "Terrestrial Magnetism," Vol. 30, p. 45.

the electric cloud, induction phenomena are produced which advance the time of beginning at other stations."

This theory would make the S.C. start at a point diametrically opposite that given by Chapman's theory, and travel in an opposite direction with a much slower velocity. The fact that both theories indicate where the earliest occurrence is to be looked for is, from the critical standpoint, a great advantage.

Fr. Rodés examined in the light of his theory the unrevised data for the Coast and Geodetic Observatory times of the fifteen S.C.s during 1906 to 1909, as well as a variety of data for more recent S.C.s, and he considered apparently that the results were rather favourable to his theory.

§ 3.

In the recent Paper by Messrs. Bauer and Peters the conclusions reached as to the easterly or westerly propagation of S.C.s at the rate required by Dr. Bauer's original theory seem to be in practical agreement with the conclusions reached by Prof. Angenheister and myself, but there is no clear explicit statement on the subject. This is the more to be regretted owing to the doubts thrown on the data which we employed. After referring to the revision of the Coast and Geodetic Observatory data mentioned above, they say (l.c., pp. 49 and 50) "the revised data were published in 'Terrestrial Magnetism,' Vol. 16, pp. 200-204, 1911. It had been the intention to follow the same procedure for other observatories in order that a discussion might be based on strictly homogeneous data; however, various matters operated to frustrate this plan. In the meantime, discussions of the published data for the 15 cases were undertaken and published by Dr. G. Angenheister and by Dr. C. Chree. *In neither case was any attempt made to use only strictly homogeneous data.* As a group, the time scalings for the four observatories in England differed more amongst themselves than those for any other similar group, as, for example, the one composed of the observatories in India and Burma. The observatories in England showed ranges, which in a number of cases amounted to from 4 to 6 minutes of time (in the case of disturbance 13*a* the *Z*-times at Greenwich and Falmouth differed by 10 minutes from one another). . . . So also Angenheister failed to note in his publication that in the case of disturbance No. 3*a* (December 21, 1906), for example, his times of beginning of the disturbance at Samoa Observatory (Apia), both for declination and horizontal intensity, . . . were systematically about 3 minutes less than the corresponding times at the Potsdam Observatory. . . ." The failure of Prof. Chapman and Fr. Rodés to employ "strictly homogeneous data" is also commented on.

§ 4.

As regards Prof. Angenheister, it may be pointed out that, since Samoa and Potsdam differ in longitude by about 175°, the phenomenon now commented on so unfavourably by Bauer and Peters would have been eminently reasonable if Dr. Bauer's original theory had been correct. As regards my own alleged shortcomings, it may be pointed out that I explicitly stated the belief that the differences in the times recorded were mainly at least of instrumental or observational origin. There was thus no more reason for me to concern myself about the differences between the four English stations (for three of which I had no responsibility) than about the differences between any other four stations near to or remote from one another.

As a matter of fact, Uccle, Val Joyeux, Tortosa and Pola, stations representative of S.W. Europe, will be found to exhibit at least as much diversity. As regards the case of S.C. No. 13, particularised by Bauer and Peters, the extreme differences from the mean time were exhibited, not by Falmouth and Greenwich, but by Falmouth (−7·4 m.) and Val Joyeux (+6·0 m.). I had explicitly called attention to this case in the following words (l.c., p. 142): "The Falmouth and Val Joyeux estimates for disturbance No. 13 appear abnormal, but were retained as they roughly neutralise one another in the mean." The method employed in my Paper—which has also been used a good deal by Bauer and Peters—was to calculate a mean time for each S.C. from the times recorded at all the stations, and use the difference of individual stations times from this in the discussion. Eighteen stations had supplied V-times ($V=Z$) for S.C. No. 13. Thus the retention or the omission of *both* Falmouth and Val Joyeux would not affect the mean time by as much as 0·1 m. Further, in any grouping of stations, Falmouth and Val Joyeux, owing to their geographical proximity, would naturally come in the same group, and the suspected data would again largely neutralise one another. In various other cases, as stated on my p. 140, I did omit suspicious data, excluding, in fact, all data from one station.

Prof. Chapman, for his part, as explained on his p. 209, omitted all times which differed as much as 2 minutes from the mean. Thus the charge of exhibiting an uncritical attitude to the data—data which, it should be remembered, had been published by Dr. Bauer himself—can hardly be fairly urged against either Prof. Chapman or me.

§ 5.

I am far from denying that greater consistency might have been obtained if all the curves had been re-measured by some one person with guidance obtained from the Potsdam curves. But it would have been difficult under such circumstances to secure perfect impartiality in the results. The fact that the data did not suffer sensibly from want of revision, so far as the immediate object in view was concerned, seems fairly obvious when we compare the mean times which I found for the several S.C.s with those now given as derived from the "homogeneous" data at Potsdam and the five Coast and Geodetic Stations (Sitka, Baldwin, Cheltenham, Vieques and Honolulu). Bauer and Peters give mean times separately, as I did, for the three elements declination (D), horizontal force (H) and vertical force (Z or V). Angenheister and Chapman confined themselves to H. Besides giving separate times for the three elements, Bauer and Peters give a composite mean time which allows weights of 3 to H, 2 to D and 1 to V. These weights are not unfairly representative of the relative importance of the elements, and this composite mean, though certain objections can be urged against it as a physical quantity, affords at least a convenient basis for comparing my mean times with those of Bauer and Peters. Calculating from my data 3-element mean times after the fashion of Bauer and Peters, the resulting times for the 15 S.C.s (algebraically) exceed those now given by Bauer and Peters by the following individual amounts: +0·4 m., −0·3 m., +0·3 m., +0·2 m., +0·2 m., +0·2 m., +0·5 m., −0·3 m., −0·1 m., −0·1 m., −0·3 m., +0·2 m., −0·3 m., −0·6 m., −0·2 m. The mean of these fifteen excesses is −0·01 m., showing that the differences may be treated as accidental. In only one case, No. 14, does my time differ by more than 0·5 m. from that of Bauer and Peters. On that occasion there is a difference of 1·2 m. between the times derived

by Bauer and Peters from the D and H curve measurements, the difference in my case being 0·6 m.

§ 6.

As was pointed out in my original Paper, the differences between the times of S.C.s derived from the D, H and V curves on an individual occasion, though containing a large accidental element, are partly of a systematic character. At the average station there is a decided tendency for the D time to be earlier than the V, and the H time earlier than the D. As shown in my Table V (l.c., p. 147), the means of these time differences for the 15 S.C.s, allowing equal weight to each station, irrespective of the number of data it supplied, were $D-H=+0{\cdot}24$ m., and $V-H=+0{\cdot}65$ m. This phenomenon cannot be assigned to the lack of "homogeneity" in my data, as it is clearly apparent in the data in Bauer and Peters' Table III (l.c., p. 53). For the testing of Dr. Bauer's original theory, or that of Fr. Rodés, which postulates a similar comparatively slow rate of propagation, the above phenomenon is unimportant.

But it is otherwise when we have to do with theories such as Chapman's, which postulate a much higher rate of propagation. The question thus demands further consideration.

An S.C. is neither infinitely large nor infinitely rapid. Also a change of curve ordinate to be perceptible must attain a certain size. Thus some interval, whether a fraction of a second, a few seconds, or a number of seconds, must elapse before an S.C. movement on a magnetic curve is recognisable. The time will naturally be shorter the larger the component of the disturbing force affecting the particular magnetograph, and the more sensitive the magnetograph. A good idea of the general character of S.C.s at different parts of the earth may be obtained from plates XIX to XXVII of the volume "Terrestrial Magnetism, British Antarctic Expedition," 1910-13. The traces of three elements are shown, and the corresponding scale values are indicated by the length of the arrows drawn adjacent to the curves, each representing 50γ. In some cases N (north component) and E (east component) are shown instead of H and D, but there is usually a close resemblance between N and H changes. It is at once obvious that the change in H is usually much the largest, especially in low latitudes. In a D magnetograph the equivalent in force of 1′ varies directly as the local value of H. In ordinary magnetographs 1 mm. of ordinate represents roughly 1′. Thus, as measurers of force, D magnetographs are apt to be insensitive instruments near the magnetic equator, where H is large. The short length of the arrows representing 50γ in the case of the Alibag (Bombay) and Honolulu D curves in the plates referred to above at once catches the eye. These considerations might explain a small lag in the times derived from the D and V curves, as compared with those derived from the H curves, but they certainly do not account for all the phenomena observed. It is true that the D-H difference is specially prominent in the "homogeneous" data from those two of Bauer and Peters' stations, where H is highest, being +0·83 m. at Honolulu and +0·58 m. at Vieques. But Bombay is a station with a still higher value of H, and for it I got D-$H=-0{\cdot}19$ m. If, moreover, we regard a station as good or bad according as the times assigned at it differed little or much from the mean of the times assigned at all the stations, Bombay was better than the average. A positive value for D-H may be, of course, a purely natural phenomenon, the disturbing force

having initially no horizontal component perpendicular to the magnetic meridian. But it is difficult to believe that this could be a general phenomenon, in view of the continual secular change and other considerations. If it were a natural phenomenon, we should at least expect similar results at stations in the same part of the world. While, however, *D-H* at Uccle on an average from 13 S.C.s was +0·42 m., at Wilhelmshaven, on an average from 12 S.C.s, it was −0·71 m. At one non-British station, which need not be specified, the mean value of *D-H* was +3·05 m., no one pifference exceeding +5·6 m. In such a case one would think there must be some deculiarity in the way of marking or measuring the times.

§ 7.

As regards a difference between the *V* and *H* times, a total absence of a vertical component at the start seems *a priori* a much less unlikely contingency than a total absence of a *D* component. *V* might again alter initially as the square of the time, while *H* altered as the first power. On the other hand, an instrumental lag of a frictional kind in the ordinary Lloyd-balance *V* magnetograph is not at all an unlikely contingency. I am afraid the *V* phenomena during S.C.s are unlikely to be satisfactorily ascertained until use is made of a horizontal coil and galvanometer, such as Dr. Crichton Mitchell* has employed at Eskdalemuir. However this may be, the fact remains that when a mean value of *V-H* was calculated for each of the 15 S.C.s, from all the stations available, a plus value was obtained for every S.C., except No. 1, for which the result was zero. When the stations were considered individually, all the S.C.s available being utilised, a negative value was obtained for two or three stations which had supplied data for only a few S.C.s; but of the stations which had supplied data for a majority of the S.C.s only one (Bombay) gave a negative value, and as it was only −0·04 m. it hardly counts. As in the case of *D-H*, there were a few stations at which the phenomenon could hardly be explained by anything but some peculiarity in marking or measuring times. In one such case, again a non-British station, the mean value obtained for *V-H* from 13 S.C.s. was +3·25 m., all the differences being positive and none exceeding +4·8 m.

§ 8.

The values of the $D \sim H$ and $V \sim H$ time differences for the "homogeneous" data employed by Bauer and Peters are naturally of special interest, because they must inevitably influence our attitude to Bauer and Peters' conclusions. These differences are given in the following table, the stations being arranged in order of geographical latitude.

TABLE I.—*Differences in Mean Times for D, H and V.*

Mean time difference.	Sitka.	Potsdam.	Baldwin.	Cheltenham.	Honolulu.	Vieques.
	m	*m*	*m*	*m*	*m*	*m*
D—*H*	+0·04	+0·01	+0·19	+0·07	+0·83	+0·58
V—*H*	+0·27	+0·89	+0·30	+0·17	+0·52	+0·32

It will be observed that the value of *V-H* at Potsdam is decidedly in excess of

* Proc. Roy. Soc. of Edinburgh, Vol. 45, p. 297.

the mean derived in my Table V (l.c., p. 147) from all the stations available. The other values, except those for *D-H* at Honolulu and Vieques, are smaller than those at the average station. It is obvious, however, that they might be rather vital when it comes to testing Chapman's theory, or any theory depending on similarly small time intervals.

§ 9.

After giving the *D*, *H* and *V* times of the 15 S.C.s at their 6 selected stations, and a variety of statistical deductions therefrom, Bauer and Peters proceed to a discussion of Fr. Rodés' theory. They conclude that the phenomena are not compatible with at all as slow a rate of propagation as this theory requires. Then, after a mere reference to Prof. Chapman's theory, they proceed in their § 22 to test the hypothesis that S.C.s more often travel easterly than westerly, and conclude that "The average speed of propagation . . . would apparently be about 1,000 km. per second eastwardly." In their § 23 they then try the hypothesis that the direction of propagation is towards the west, and on this hypothesis find that "the apparent speed of westwardly propagation is about 800 km. per second." In their § 24 the authors proceed to say "But thus far we are unable to determine definitely in which direction, on the average, the abrupt disturbances (their name for S.C.s) are actually propagated. . . . Finally the desired clue was obtained by computing the mean magnetic latitudes of each group [they had arranged the 6 stations in groups of 3 or 2] . . . whether we grouped the observatories according to increasing east longitude, or according to increasing west longitude, in either case the magnetic latitude was increased in going from the first to the second group [i.e., from the group giving the earlier to that giving the later mean time for the S.C.s]. A preliminary examination indicates that this fact was the chief cause for the conclusion reached in 1910" (already given above). A discussion of the hypothesis that S.C.s "progress generally according to increasing magnetic latitude" follows in their § 25.

§ 10.

Before discussing the results further, the use of the term "magnetic latitude" requires explanation. Bauer and Peters' magnetic latitude φ' is really defined as $\varphi'=\tan^{-1}(\frac{1}{2}\tan I)$, where I is the true dip. The term has a precise physical significance in the case of a uniformly magnetised sphere. If $\varphi=\pi/2$ represents the magnetic axis of such a sphere, it is easily proved that

$$\tan I=2\tan\varphi.$$

As is well known, the first order terms in the Gaussian analysis of the earth's magnetic potential are much the largest, and the earth's magnetism, so far as expressed in these terms, is the same as that of a uniformly magnetised sphere whose axis is inclined to the axis of rotation (geographical axis) at some 12°, the northern end of this axis being not far from 70°*W*. It is not unusual to speak of the axis of the first order Gaussian terms as the earth's magnetic axis, etc. When this is done, we naturally think of lines of equal magnetic latitude as small circles on the earth's surface, having their centres on the magnetic axis, and otherwise analogous in every way to lines of geographical latitude. But the first Gaussian harmonic supplies only a first approximation to the earth's real magnetism, and it is a somewhat rough approximation when it comes to the question of the true position of the magnetic poles,

defined as the places where the dipping needle is vertical. If we define φ' as Bauer and Peters do, we must take the earth's true isoclinals as lines of equal magnetic latitude. The complications that ensue will be obvious to anyone who considers even the smoothed isoclinals which do duty on world charts. The geographical distance between two given isoclinals is a very variable quantity at different parts of the earth's surface. If the time of passage of an S.C. from one isoclinal to another is invariable—as is implied by Bauer and Peters' calculations on their pp. 59-61—the real velocity of propagation must be very variable. When we pass to true isoclinals there are often large local irregularities. There are even in places small complete ovals. Thus, if we had transmission strictly following the direction of increasing dip, we should in some adjacent regions not near the equator have propagation in diametrically opposite directions.

§ 11.

The results which appear much the most favourable to this final hypothesis of Bauer and Peters are those given in their Table XI (l.c., p. 59), as derived from the *H* times of the 15 S.C.s at their six stations. These are given below in Table II. δT means the departure of the time given by the individual station from the mean of the times at all six stations. The information as to the geographical co-ordinates has been added to give an idea of the zig-zag nature of the motion which is suggested by the values of δT, if accepted as physical facts.

TABLE II.

Station.	Lat. *N.*	Long. *W.*	φ'	δT
	°	°	°	*m*
Honolulu	21·3	158·1	22·7	−0·74
Vieques	18·1	65·4	30·4	−0·27
Potsdam	52·4	346·9	48·7	+0·05
Baldwin	38·8	95·2	52·2	+0·12
Cheltenham	38·7	76·8	54·7	+0·14
Sitka	57·0	135·3	61·2	+0·67

The formula $\delta T = x\delta\varphi'$, with x the velocity per 1° of φ', applied to the data in Table II by Bauer and Peters, is said to show a correlation coefficient of 0·96.

In a second investigation the authors employed the less homogeneous results of all the (29) observatories arranged in eight groups according to magnetic latitude. In this case *V* supplied the results most favourable to the hypothesis. Stations with southerly dip appear in the same group as stations with corresponding northerly dip, in strict accordance with the view that the direction of propagation is from the (magnetic) equator towards either pole. In both these investigations the times derived from *D* gave very poor support to the theory. Some *H* data were also considered from an additional 15 S.C.s recorded in 1922 to 1925. On their p. 67 the authors summarise their results as follows :

" § 34. Such evidence as could be obtained from the data at the observatories chiefly considered in this Paper (suggest that ?) the speed of progression of an abrupt disturbance (i.e., an S.C.) around the earth may be 1,000 kilometres per second, or more. The average speed of progression in magnetic latitude, from the combined

testimony of the 30 disturbances, would be about 100 kilometres per second, so that if an abrupt disturbance progressed uninterruptedly from the magnetic equator to the magnetic pole it would take about 1·8 minutes. There may also be cases in which an abrupt disturbance will occur everywhere simaltaneously in so far as can be determined with the instrumental appliances in general use. § 35. We regard at present the foregoing ideas chiefly as a working hypothesis to be tested with the aid of additional material. . . . ."

§ 12.

It may be noted that an equatorial velocity of 1,000 km. per second would give about 40 secs. for the circumnavigation of the earth. As this seems the lowest velocity now claimed for east~west propagation, we may presumably regard Dr. Bauer's original view that S.C.s perambulated the globe in from 3 to 7 minutes as definitely abandoned. But this seems the only definite result reached.

If the true interpretation of the " working hypothesis " is that an S.C. originates simultaneously everywhere in the magnetic equator, and thence travels towards both true magnetic poles, this would imply in the North Atlantic and in Western Europe a geographically western component, and in the southern Atlantic an easterly component. In a large part, however, of Asia, Eastern Africa and Eastern Europe the direction of propagation would be nearly in the geographical meridian, and in most areas there would be a considerable component in that direction. It would thus appear worth while considering what conclusions I drew from my original investigation into the possibility of a north-south direction of propagation, though it is not referred to by Bauer and Peters. It is true that I assumed that the direction of propagation was the same and not opposite in the northern and southern hemispheres. But this could not make much difference to the result, as only three of the stations I employed had southerly dip, and they were all in low latitudes. The following is the conclusion I drew for H from the data of 30 stations (l.c., p. 141) : " So far as H is concerned, there seems no more evidence of motion of disturbances (S.C.s) along meridians than there was along parallels of latitude." In the case of V only 26 stations were available, and some of these gave data for a minority of the S.C.s. The conclusion drawn (l.c., p. 142) was as follows : " In disturbance No. 1 there is a suggestion of motion from south to north, the first seven entries (station differences from mean time, the stations being arranged according to decreasing northern latitude) being all plus and the last six all negative. The numerically largest values, however, both positive and negative, occur in intermediate latitudes, and the phenomenon may be quite accidental."

§ 13.

In view of Bauer and Peters' conclusions, especially their suggestion of oppositely directed propagation in the two hemispheres, I have looked at the data again, and give in Tables III and IV some data which appear to throw some further light on the subject.

Table III reproduces some of the results I had obtained for the differences between mean times for H, D and V at the individual stations, and at all the available stations combined. The last column, containing a combined mean which allows weights of 3 to H, 2 to D and 1 to V, is new. The data assigned to those of Bauer

and Peters' stations included differ slightly from those deducible from Bauer and Peters' own figures, because my all-station means differ from theirs.

TABLE III.—*Individual Station Time Differences.*

Station.	Latitude.		Difference from Mean-station time (minutes).			
	Geog.	Mag.	*H*	*D*	*V*	3 elements.
	°	°				
Sitka	57 N	61 N	+0·73	+0·48	+0·27	+0·57
Ekaterinburg ...	57 ,,	55 ,,	+0·78	+0·65	+1·08	+0·79
Stonyhurst	54 ,,	52 ,,	−0·74	−0·77	−1·00	−0·79
Wilhelmshaven ...	54 ,,	51 ,,	−0·34	−1·21	...	−0·69
Potsdam ...	52 ,,	49 ,,	+0·11	−0·12	+0·35	+0·07
Greenwich	51 ,,	50 ,,	+0·47	+0·36	+0·12	+0·37
Helwan	30 ,,	23 ,,	+0·03	+0·84	+0·59	+0·39
Honolulu	21 ,,	23 ,,	−0·68	−0·09	−0·81	−0·50
Bombay	19 ,,	11 ,,	−0·24	−0·67	−0·93	−0·50
Vieques	18 ,,	31 ,,	+0·11	+0·55	+0·13	+0·26
Kodaikanal	10 ,,	2 ,,	+0·81	−0·25	+0·24	+0·36
Buitenzorg	7 S	17 S	−0·25	−0·55	−0·76	−0·43
Samoa	14 ,,	16 ,,	−0·18	−0·30	+0·85	−0·05

It will be obvious to any mathematician on inspection of Table II that the conclusion drawn by Bauer and Peters from the *H* data, as to the motion from south to north magnetic in the northern hemisphere depends mainly on the two stations of extreme magnetic latitude, Sitka and Honolulu. On looking at Table III it will be seen how peculiarly favourable to the theory the choice of these two stations happened to be. Ekaterinburg, it is true, might have been as favourable as Sitka, and Bombay in the case of *V* and *D*, though not in the case of *H*, as favourable as Honolulu. Also Greenwich might have been as favourable as Potsdam. But the substitution of Stonyhurst or Wilhelmshaven for Sitka, and of Kodaikanal or Helwan for Honolulu would have made all the difference in the world to the results. In the case of *V* the opposition presented by Buitenzorg and Samoa is most striking. We might, in fact, select six stations which would lead to results diametrically opposed to those of Bauer and Peters.

§ 14.

Inspection of my Table VII (l.c., p. 151) did, however, suggest that if we concede the possibility of propagation along geographical meridians, or along curves orthogonal to isoclinals, more is to be said for propagation to the north than for propagation to the south in the northern hemisphere. The stations were divided into two groups, according as the station time was earlier or later than the all-station mean time. This was done for *H*, *D* and *V* separately. The results are given in Table IV, magnetic latitude having the meaning assigned it by Bauer and Peters. When either group of stations contained a station or stations having southerly latitude, two mean latitudes were calculated, the one arithmetical (unbracketed), the other algebraic (bracketed). The former figure should be employed in connexion with Bauer and Peters' hypothesis. The latter gives velocity the same direction S. or N. everywhere. A difference between the mean latitudes found for the two groups of stations is favourable to a finite rate of propagation, the result being the more favourable the larger the difference between the two mean latitudes. The results, it will be seen, are all favourable to propagation from the magnetic equator or from south to north in the northern hemisphere. The difference figures in brackets for

the two mean latitudes are in excess of the unbracketed. This is at least not unfavourable to the view that the velocity has the same direction in the two hemispheres.

Whether we take geographical or magnetic latitude, the element the results from which appear most favourable to the hypothesis of propagation with a finite velocity is V, and as we have already observed it is the element instrumentally most

TABLE IV.—*Mean Latitudes of Groups of Earlier and Later Stations.*

Group of stations.	Mean geographical latitude.			Mean magnetic latitude.		
	H	*D*	*V*	*H*	*D*	*V*
	° °	° °	° °	° °	° °	° °
Earlier...	36·0(28·1)	36·7(29·8)	30·0(23·6)	33·5(26·3)	33·7(27·5)	27·5(22·3)
Later	39·7	39·4	41·9(39·9)	40·4	41·3	41·9(39·6)
Latitude difference ...	3·7(11·6)	2·7 (9·6)	11·9(13·3)	6·9(14·1)	7·6(13·8)	14·4(17·3)
Time difference	0·85 m.	0·82 m.	1·43 m.	0·85 m.	0·82 m.	1·43 m.

open to suspicion. Even in the case of V the results do not really afford much support to Bauer and Peters' theory. If 90° in latitude were really covered in 1·8 m., we should expect a much greater difference between the mean latitudes from the two groups of stations in view of the large differences between the corresponding mean times.

§ 15.

A theory which suggests propagation from the equator to the poles is much more difficult to test satisfactorily than one which suggests propagation completely round the earth, or from one end of an equatorial diameter to the other. There are, in the first place, only 90° from equator to pole (geographical or magnetic). Then there are few magnetic observatories in either equatorial or high latitudes. Of the 30 observatories included in my Paper only one had a magnetic latitude less than 10°, and only three had magnetic latitudes greater than 55°. Bauer and Peters' six observatories gave a range of magnetic latitude of only 38·5°.

To test satisfactorily a rate of propagation from the equator such as 90° in 1·8 m. by means of records from existing observatories obviously calls for higher time accuracy than suffices to test either Dr. Bauer's original theory or that of Fr. Rodés. This is all the more true because—as the Antarctic volume referred to above clearly shows—S.C.s change their type with magnetic latitude. At stations like Bombay, Mauritius or Helwan, in lower latitudes, the H movement, which is far the most important, seems normally to be unidirectional (an increase of H); whereas in higher latitudes—e.g., at Eskdalemuir or Sitka—it is generally, if not always, oscillatory. Again, curves in higher latitudes are much more subject than those in lower latitudes to irregular small oscillations, and the presence of these may make it difficult, if not impossible, to say exactly what the S.C. movement has consisted of.

Another consideration is that Bauer and Peters' latest hypothesis may easily undergo transformation. The idea of a disturbance propagated from the equator to both poles may recommend itself to those who suppose, with Prof. Störmer and Dr. Van Bemmelen, that a ring of ions in the upper atmosphere over the magnetic equator is responsible for some of the phenomena of magnetic storms. But, obviously, others might think the S.C. more likely to originate in one or in both auroral zones. We might thus have variants of the theory, some postulating movement invariably to or from the magnetic equator, others postulating the same direc-

tion of motion whether north or south all the way between the auroral zones. If, moreover, a velocity as low as 90° in 1·8 m. proved untenable, an enhanced velocity might be suggested.

§ 16.

My principal reason for discussing the matter at such length now is that, at the last meeting of the International Union of Geodesy and Geophysics, a sum of money was voted to the Section of Terrestrial Magnetism and Electricity for the purpose of installing at a few stations special apparatus for ascertaining whether S.C.s do or do not undergo propagation at a finite rate. It will, I think, be generally agreed that, whatever theory or theories there may be to test, the same magnetic element or elements should be recorded at the special stations. Financial considerations may require a single element to be selected, and an early decision on that point may be necessary. The stations most suitable for the purpose have also to be considered.

For ordinary purposes the *D* magnetograph has much to recommend it. It is the simplest to construct. Its scale value is invariable, and there is no temperature coefficient. On the other hand, for this particular enquiry the *H* magnetograph has special advantages. It is easier to determine accurately the commencing time of a large than of a small movement, and in the case of S.C.s the *H* movement is usually much the largest. Again, as explained above, whatever the reason, the commencing times found for S.C.s from the *H* curves tend to be earlier than those derived from the *D* or *V* curves. Thus we should naturally feel more confident of having timed the real commencement if we used an *H* magnetograph than if we used a *D* or a *V* magnetograph. To test any theory such as Bauer's original theory, or those of Chapman and Rodés, in which the direction of propagation is approximately east and west, it is obviously desirable, if an *H* magnetograph is used, to select stations in which the *H* S.C. movement is of a common type. This suggests that the stations should be in a comparatively low latitude. A comparison of records from a number of possible stations would be a desirable preliminary to a final selection of stations. Bauer's original theory may require no further testing, but there remain Chapman's and any number of possible intermediates between it and Rodés' theory.

It would seem desirable to deal thoroughly first with east-west propagation. The test of south-north propagation promises to be a more difficult job, and it would be well in any case to allow further time for the criticism of Bauer and Peters' last theory or for the proposal of modifications of it.

Local characteristics seem to play a greater part in the vertical component than in the horizontal components during ordinary magnetic disturbance, and the same may be true even for S.C.s. Thus an investigation of the *H* phenomena *may* not exhaust the subject. But in the case of the ordinary *V* magnetograph it is difficult to feel sure that the record of rapid changes of force is free from instrumental peculiarities, and it might be well to await the development of instruments employing a horizontal coil and a galvanometer.

DISCUSSION.

Mr. F. E. Smith said that the expression "homogeneous data" appears to mean the same as "arbitrarily selected data," and physicists are indebted to the author for his strenuous insistence on a rigorously scientific examination of the theories that have been put forward. The type of apparatus required for settling the questions at issue would depend on the theory to be tested. A theory which postulates a comparatively slow rate of propagation of the sudden commencements could very easily be tested with the aid of wireless time signals, but if the time differences between the sudden commencements at different stations be very small, the problem becomes much more difficult.

VI.—THE MEASUREMENT OF TEMPERATURE BY THERMOCOUPLES IN UNEQUALLY HEATED ENCLOSURES.

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Received July 7, 1925.

(*Communicated by* PROF. GWILYM OWEN.)

ABSTRACT.

In measuring temperature by thermocouples in unequally heated enclosures it is shown that the recorded temperature depends upon the thickness of the couple, the nature of the walls of the enclosure and the nature of the gas. An explanation is suggested based upon the fact that the amount of radiation received and emitted by a couple depends upon its position in the enclosure, and that the amount of energy transferred to it by molecular impact varies with the nature of the gas.

INTRODUCTION.

IN a Paper on "The Temperature Gradient in Gases at Various Pressures," Mandell and West,* using a chamber consisting essentially of two large horizontal, parallel plates, the upper plate being kept at a constant high temperature, whilst the lower was kept cool, showed that with two thermocouples of different diameters there was a small, but definite and systematic, difference in the temperatures recorded by them in air at atmospheric pressure. Two comparatively thick couples (diameters 0·0126 and 0·0209 cm.) were used and, for a temperature difference of 130°C. between the two plates, the readings of the couples varied by about $\pm 1\frac{1}{5}$°C. Henderson† using a single couple showed that the recorded temperature appeared to be too low near the hot plate and too high near the cold plate and attributed this to a radiation effect.

The experiments described in the present Paper were devised to investigate this interesting phenomenon of different recorded temperatures by different couples. It was thought that by using couples varying considerably in thickness, and with a much larger temperature gradient in the gas, the effect would be increased. The experiment therefore resolved itself into determining as accurately as possible the difference in temperature recorded by thermocouples when placed side by side in the same horizontal plane in the gas. It was also thought possible that the nature of the gas and the nature of the surfaces of the enclosure might have some influence on the results.

If the temperature differences varied in a uniform manner with the thickness of the couples, it was sought to find how they differed in temperature from a couple of zero diameter by a method of extrapolation first suggested by Kreisinger and Barkley.‡ As will be seen later, the results reveal the interesting and important

* Proc. Phys. Soc., Vol. 37, p. 20 (1924).

† Phys. Review, Vol. 50, p. 26 (1920).

‡ Bull. U.S. Bureau of Mines, 145 (1918). Kreisinger and Barkley measured the temperature of a stream of air flowing through different types of boilers.

facts that in an unequally heated enclosure in which convection is eliminated, the temperature recorded by a thermocouple depends upon its thickness, the nature —bright or black—of the walls of the enclosure, and also upon the nature of the gas in which it is placed.

Apparatus.

The apparatus used was practically the same as in the previous Paper,* excepting that the sides of the chamber consisted of a glass cylinder 1·95 cm. long. It will be seen, therefore, that we have in effect a layer of gas some 2 cm. thick enclosed between two horizontal plates—under conditions in which convection, and the influence of the sides on the main body of the gas may be assumed negligible, and in which the lines of flow of heat, at any rate, through the central portion of the layers, may be assumed vertical.

A movable, horizontal, porcelain platform was used as before, and now carried four thermocouples of different diameters.

Preparation and Calibration of the Thermocouples.

The thermocouples were of copper and constantan and for convenience are denoted by the terms (α, very thin, diam. 0·0044 cm.), (β, thin, 0·0123 cm.), (γ, thick, 0·0195 cm.) and (δ, very thick, 0·0315 cm.).

They were made by fusing the wires together "end-on" in a reducing flame. Several hundred couples were made until specimens were obtained showing no blob at the junctions when examined under a microscope. When fixed their general appearance was that of four almost coplanar parallel cylinders of equal lengths. The couples were placed on the platform in the order δ, β, γ, α, that is, not in the same order as their thickness, the distance between the two outside couples being 1·2 cm.

In regard to the couples on the hot and cold plates, a convenient method had to be devised so that they could be detached easily for re-calibration without unsoldering them, as this might alter their e.m.f.s. Small, thin, circular-brass discs of about 1 cm. diameter and of the same material as the hot and cold plates were employed. The couples were "hard-soldered" on to the brass discs, which were then screwed into holes in the hot and cold plates at a distance of about 1·5 cm. from their centres until the surfaces of the brass discs were level with the plane experimental surfaces.

Since the *differences* of temperature to be measured were only of the order of a few degrees, all the couples had to be very carefully calibrated and an accurate method of reading to $\frac{1}{10}$°C. had to be devised.

Before the six couples were actually fixed in the apparatus they were calibrated in an electrically heated black-body enclosure against a mercury-in-glass thermometer. It was found that on keeping them at a temperature of 160°C. there was a gradual change in their e.m.f.s for about 20 hours. The actual calibration was not begun until after heating for about 50 hours. The e.m.f.s of all the couples were then read for 28 different temperatures, a considerable time having to elapse between each set of readings, until the temperature was absolutely steady.

* Loc. cit.

As was to be expected, for any fixed temperature, the e.m.f.s of the couples were not quite the same and, in fact, were found to be in the decreasing order α, γ, δ, β. A graph of such large dimensions was required that it was considered that the readings for 28 different temperatures were not sufficient, so the following method of graphing was employed. The temperatures of the enclosure were plotted as abscissæ and the *differences* of e.m.f. between the very thin couple and the other platform couples were plotted as ordinates, the result giving three parabolas. With the help of this graph the proper e.m.f.-temperature graph for all the couples could be constructed after drawing the smooth curve through the 28 points for the very thin couple. It follows that this method was especially accurate for the measurement of small temperature differences. The couples were recalibrated after using for six months and at other times, and had not changed to any measurable extent.

Method of Carrying Out an Experiment.

The apparatus was filled with the dry gas at atmospheric pressure. The hot and cold plates usually took three to four hours to attain the temperatures chosen, before steady conditions were obtained. The platform thermocouples which had previously been placed as close to the hot plate as possible were then gradually moved down by measured steps, until the cold plate was reached. At each step the e.m.f.s registered by all the thermocouples, including those on the hot and cold plates, were noted. These observations usually took one and a half to two hours, since for each movement of the platform it was necessary to await steady conditions.

Results.

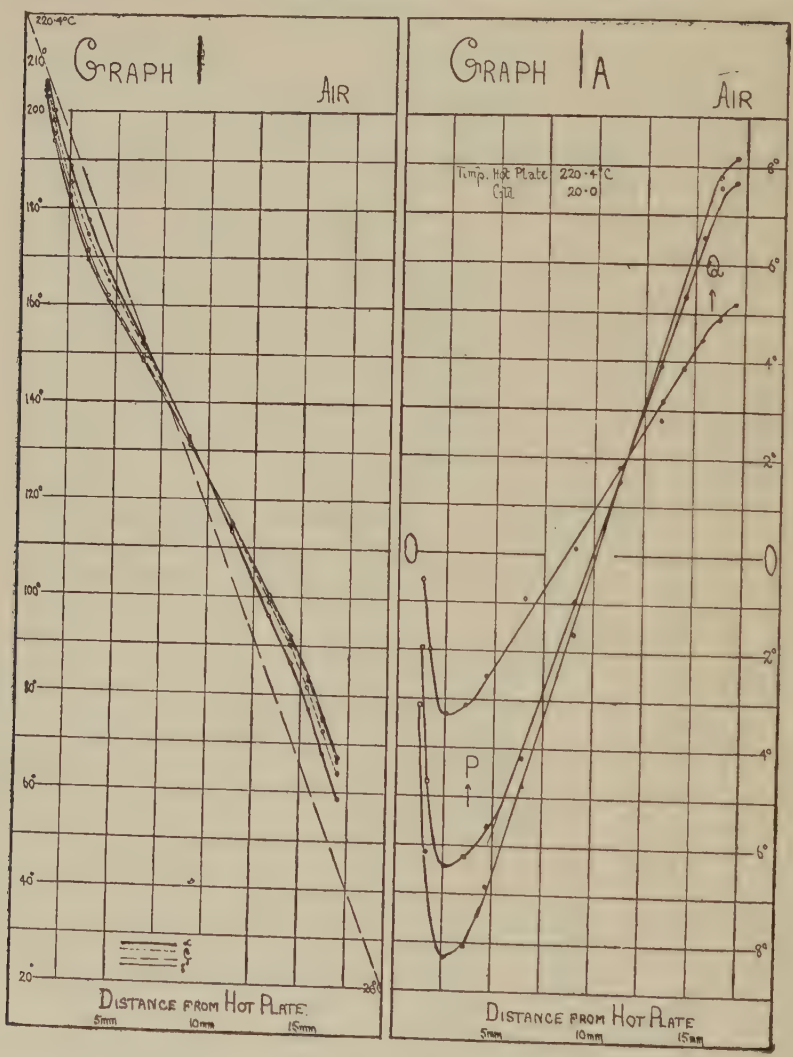
The first series of graphs records the results obtained with gases at atmospheric pressure, the hot and cold plates having highly polished brass surfaces. In this series of experiments the distance of nearest approach of the couples to the hot plate was 1·25 mm.

Graph 1 is typical of about twenty experiments obtained with air, and is constructed from the data of the first four columns of Table I. The hot plate was at a

Table I (*Graphs* 1 *and* 1a).

Brass Surfaces.			Air.	Atmospheric Pressure.	Temperature { Hot plate, 220·4°C. Cold plate, 20·0°C.			
					Differences (°).			Distance in mm. from hot plate.
α V. Thin.	β Thin.	γ Thick.	δ V. Thick.		α and β	α and γ	α and δ	
206·35	205·8	204·4	203·25		—0·55	—1·95	—3·1	1·25
200·00	198·0	195·3	193·85		—2·0	—4·7	—6·15	1·70
188·7	185·0	182·25	180·4		—3·3	—6·45	—8·3	2·60
177·5	174·4	171·35	169·4		—3·1	—6·25	—8·1	3·55
167·5	165·0	161·9	160·65		—2·5	—5·6	—6·85	4·65
153·3	152·4	149·1	148·5		—0·9	—4·2	—4·8	6·50
132·95	133·1	132·0	131·3		+0·15	—0·95	—1·65	9·10
113·45	115·3	115·25	114·9		+1·85	+1·8	+1·55	11·30
96·25	99·05	99·45	100·15		+2·8	+3·2	+3·9	13·30
86·5	90·4	91·85	91·9		+3·9	+5·35	+5·4	14·45
76·95	81·4	83·5	83·8		+4·45	+6·55	+6·85	15·35
67·5	72·4	75·1	75·3		+4·9	+7·6	+7·8	16·20
58·7	63·9	56·4	66·9		+5·2	+7·7	+8·2	17·00

temperature of 220·4°C., and the cold plate 20·0°C. The dotted straight line joining these temperatures would represent a uniform temperature distribution between the plates. The experimental curves, however, as can be seen, deviate markedly from such a line, and furthermore, the temperature distribution as recorded by the couples varies in a regular manner with the thickness of the couples. It will be seen that the



thinnest couple reads the highest temperature near the hot plate and the lowest temperature near the cold plate.

Graph 1A is also drawn from Table I, where columns 5, 6 and 7 give the differences in temperature between the α and β, α amd γ and α and δ couples respectively. It is a simple method of representing clearly how the temperature differences of the couples of Graph 1 vary across the chamber. The ordinates from 0—0 represent

ne temperature differences in degrees between the very thin (α) and the other platform couples, the ordinate being drawn below 0—0 if the temperature be lower, and above, if higher than that of the thinnest couple. It will be noticed in this and similar graphs that three straight lines are obtained which bend sharply when a few millimetres from the hot and cold plates.

When a result similar to Graph 1A was first obtained showing, as it does, a difference of ± 8°C. between the α and δ couples, it was thought that perhaps heat was being conducted from the wires, the thicker couples being, of course, the better conductors. On referring, however, to Fig. 1 it is seen that about seven centimetres of the thermocouple wires lie on each arm of the platform—that is, in the same horizontal plane where the temperature is uniform over a considerable area, so that there would be little tendency for loss of heat by this means. Besides, no explanation of the effects of conduction would account for the peculiar bend of the curves of temperature distribution in Graph 1, or the bends in the temperature difference curves of Graph 1A.

The next question to decide was whether these differences were due, in some ways to the order in which the couples were placed on the platform. At different times (owing to breakages) the couples were fixed in different order, but similar results were always obtained.

It is to be noted, too, that the differences recorded do not follow the natural variations in e.m.f. of the couples, which, as pointed out on page 49, take place at any constant temperature.

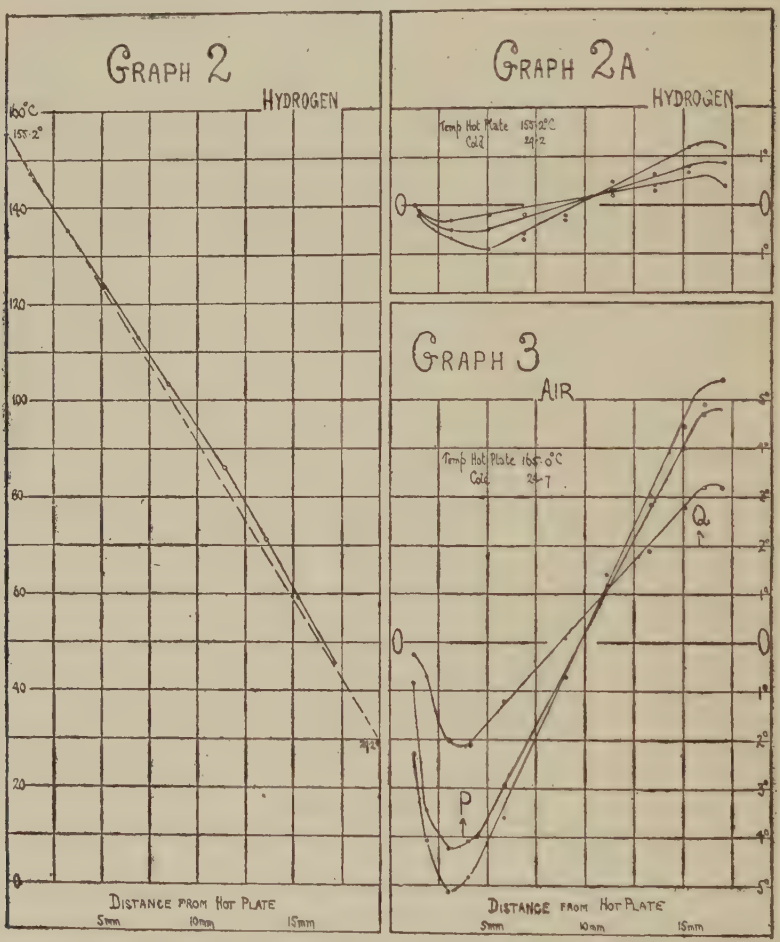
Any errors arising from movements of the platform by sagging due to time are ruled out by the fact that identical results were obtained with any particular gas after the apparatus had been used for several weeks as were obtained the first time the apparatus was used. Furthermore, readings taken with two very thin couples showed that the platform remained not only rigid, but also truly horizontal. Additional proof that the temperature differences recorded by the couples is an effect not to be attributed to conduction along the wires, nor to warping of the platform, is shown by the fact that different gases give different results.

Graph 2 was obtained for the very thin couple when hydrogen from a gas cylinder replaced the air in the chamber, the temperatures of the hot and cold plates being 155·2°C. and 29·2°C. respectively. From this graph it is seen that there is scarcely any "dip" in the curve in front of the hot plate. The differences between the couples were so small that they cannot be represented conveniently on Graph 2, and are given on Graph 2A, which shows that the four couples in hydrogen record readings which do not vary by more than about ± 1°C.

Graph 3 obtained with air, may be compared with Graph 2A for hydrogen. The differences in temperature between the plates in the two experiments were not the same, being 140·3° and 126·0°C. respectively, but this would not account for the striking differences in the results obtained with the two gases.

A series of readings was then taken with air at atmospheric pressure, the object being to see how the temperature differences between the couples varied with the temperature gradient in the gas, and by extrapolation, what would be the temperature recorded by a couple of zero diameter. The results obtained are shown in Graphs 1A, 3, 4, 5, 6, the differences in temperature between the hot and cold plates being respectively 200·4°, 140·3°, 166·7°, 99·6° and 62·7°C. A series of straight lines of different slopes was obtained. It will be seen that the measurement of the dis-

tances of the central axes of the couples relative to a horizontal plane, say, of the platform, must be measured very accurately. Any error in this measurement for a couple would displace one of the straight lines of the above graphs parallel to itself. Hence, since there may be a slight uncertainty as to the exact point where these

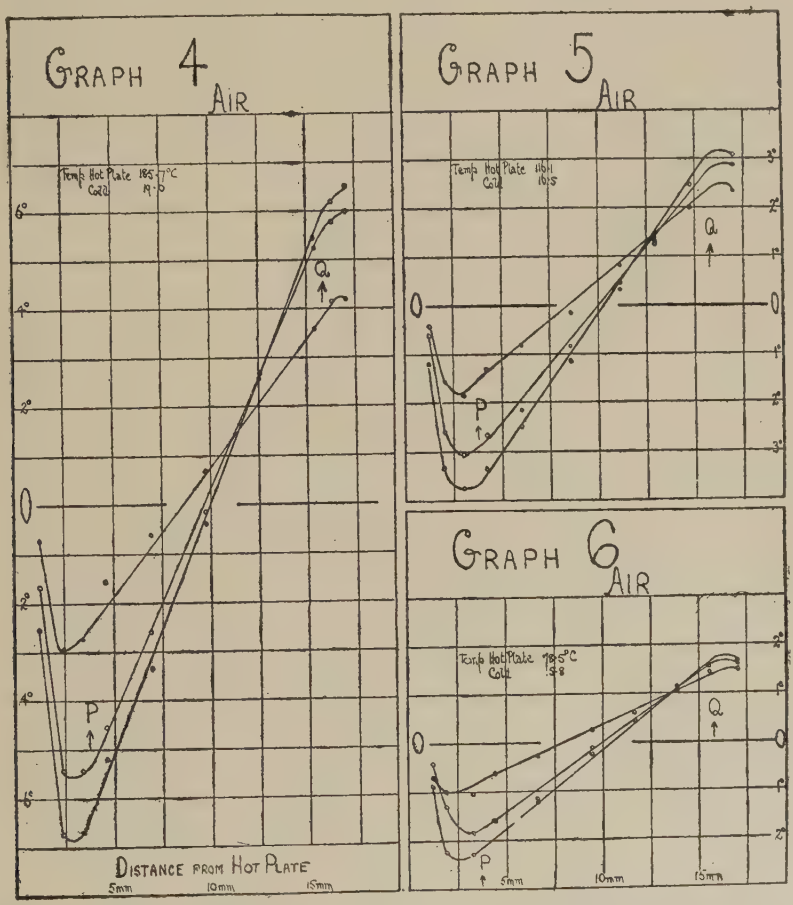


lines cross, it is preferable to take the *total* temperature differences for the couples considered at two points P and Q on the straight lines 3·75 mm. from the plates.

Graph 7 shows the method of extrapolating to zero diameter. The diameters of the couples are marked as abscissæ, the measured thicknesses of α, β, γ and δ being shown by the points W, X, Y and Z. Ordinates below these points in 0—0 denote the *total* differences in degrees between the very thin couple (α) and each of the couples β, γ and δ at the points P and Q. A series of curves was obtained passing through four points for each gradient, and by producing them through the point W the positions could be found where they meet the axis for zero thickness.

Graph 8 was obtained from Graph 7, the temperature differences between the

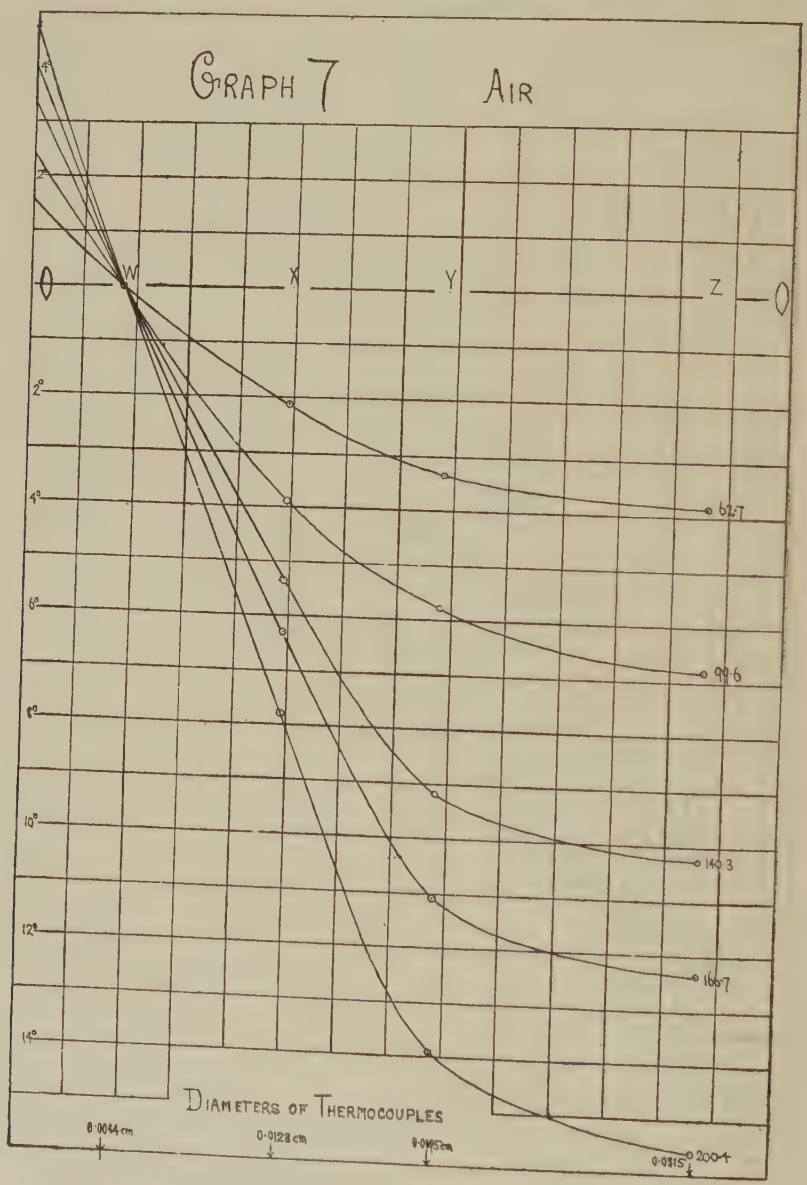
ot and cold plates being shown as abscissæ, whilst the ordinates are the *total* varia-ons in degrees between the infinitely thin and the several couples, between the oints P and Q. It is seen from Graph 8, that, when the plates differ by 200°C., ne *total* difference between the couple of zero diameter and the (α) couple between nese points is 4·8°, for the (β) couple 12·5°, for the (γ) 18·7°, and for the (δ) 20·4°C. lso the four curves obtained show that these differences are almost linear functions f the temperature differences between the hot and cold plates. Further, it is to be oted that with the α, β and γ couples, that is, up to a diameter of about 0·020 cm.,



the difference varies fairly uniformly with the diameter, but afterwards increases very slowly with increase of thickness, for at 200°C. where the difference for the γ couple (diameter 0·0195 cm.) is 18·7°, for the δ couple (diameter 0·0315 cm.) it is only 20·4°C.

If it be assumed that the couple of zero thickness indicates the correct temperature, it is interesting at this point to refer again to Graph 1 to see the effect of applying the necessary correction. It has been pointed out already that the *total* correction to be applied to the very thin couple between the points P and Q would be 4·8°,

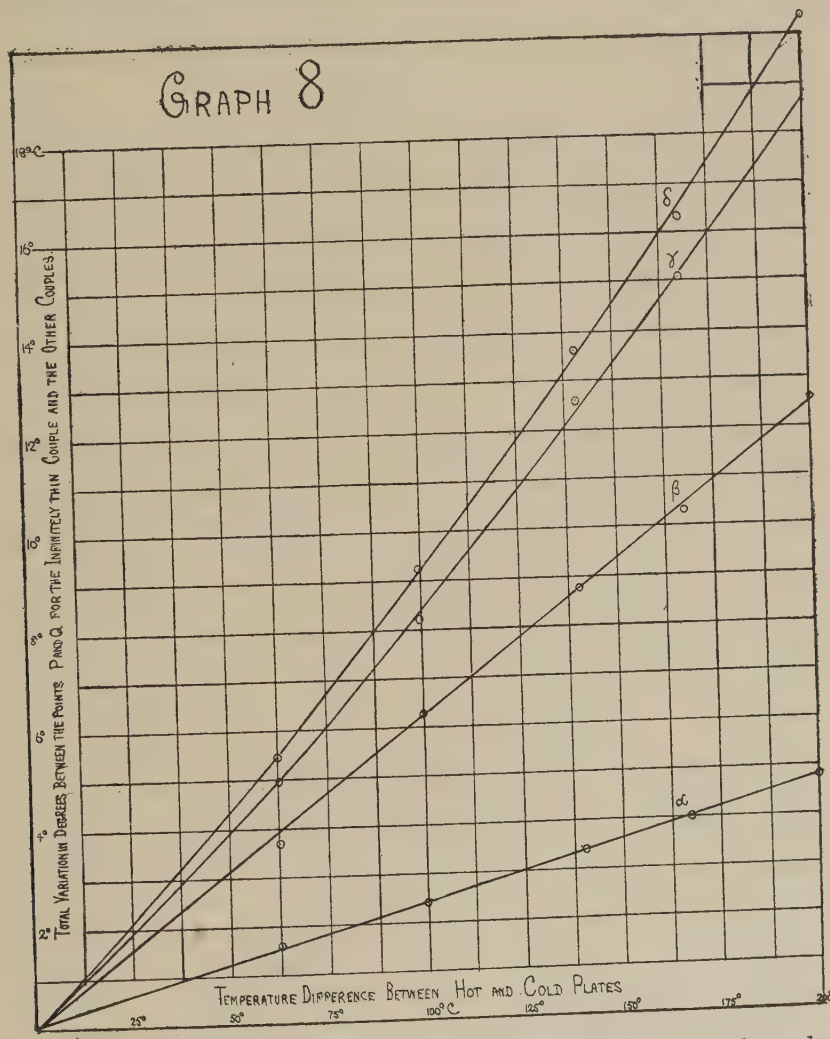
but at P a correction of about 7° and at Q about 14°, that is, a total correction of 21°C., would be required in order to obtain the uniform temperature distribution as represented by the straight line. It follows, therefore, that with air at atmospheric



pressure, if the infinitely thin thermocouple records the true temperature there is a considerable temperature "fall" in front of the hot plate, and a "rise" in front of the cold plate. This was found to be the case with air and carbon dioxide for all tem-

erature differences of the plates, and as will be shown later the gradient across the nain portion of the gas for carbon dioxide was less than for air.

Bright Nickel Surfaces.—Experiments were next carried out with highly polished ickel surfaces on the horizontal plates. The couples were re-calibrated and in e-assembling the apparatus the very thin couple was broken. It was very fragile, lue to its extreme thinness, and became considerably more so on prolonged heating.

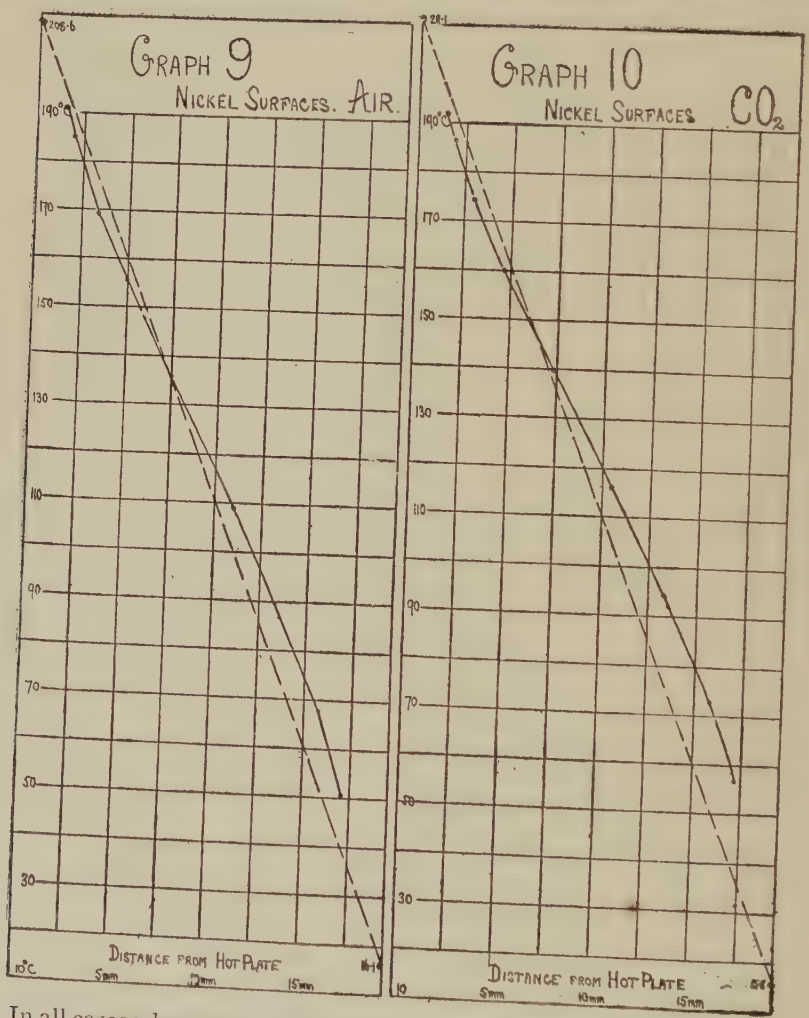

GRAPH 8
TOTAL VARIATION IN DEGREES BETWEEN THE POINTS P AND Q, FOR THE INFINITELY THIN COUPLE AND THE OTHER COUPLES
TEMPERATURE DIFFERENCE BETWEEN HOT AND COLD PLATES
δ
γ
β
α

Another similar couple was fixed on the platform and calibrated, the order of the couples now being δ, β, α, γ. A new calibrated couple had also to be fixed on the hot plate.

Graphs 9 and 10 were obtained using the new (α) couple in air and carbon dioxide respectively, the temperatures of the plates being almost the same, viz., 208·6° and 16·1° and 211·1° and 15·8°C. in the two experiments. A comparison of

the two shows one feature of importance in that the temperature slope in carbon dioxide is less than in air under similar conditions and that the temperature of the carbon dioxide is lower near the hot plate and higher nearer the cold plate than in the case of air. This point is also made very clear by Graph 11, giving two other sets of readings for the (α) couple in air and carbon dioxide for temperatures of 201·5° and 16·2° for air, and 201·2° and 16·6°C. for carbon dioxide.

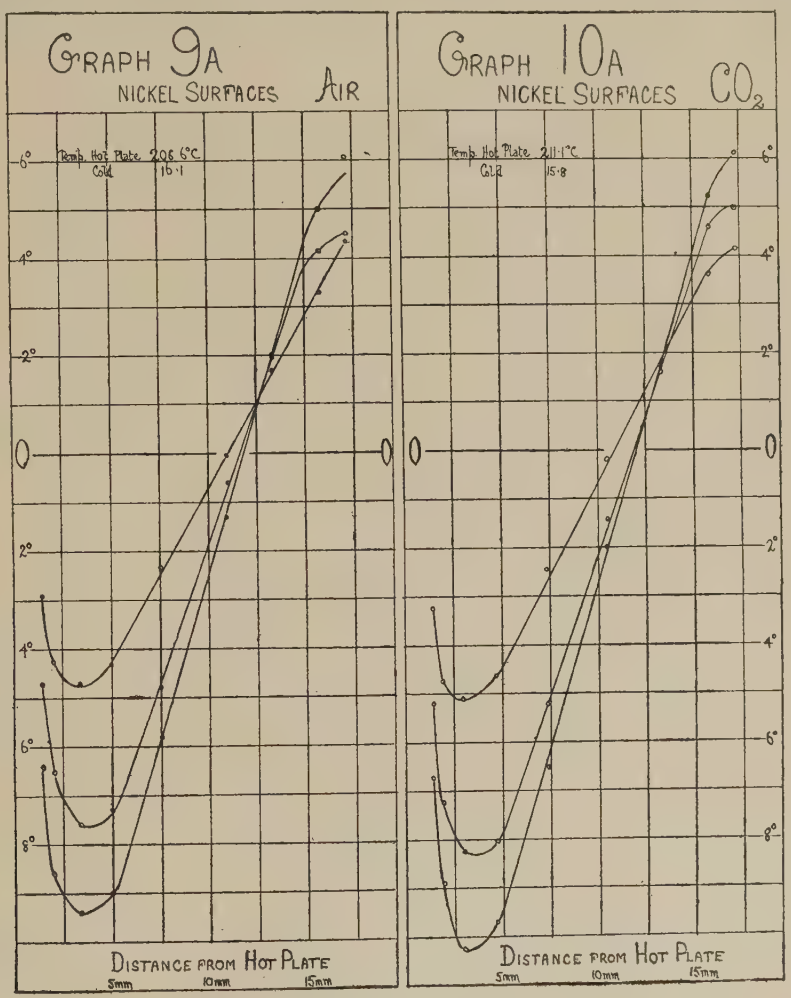
Graphs 9A and 10A are graphs of differences relating to the curves of Graphs 9



and 10. In all cases when comparing these two gases, it was found, as in these graphs, that the slopes of the line of temperature differences were slightly greater in carbon dioxide than in air. This would mean that if graphs similar to 7 and 8 were constructed for carbon dioxide the differences between the infinitely thin couple and the several couples would be slightly greater in carbon dioxide. On comparing Graphs 9 and 10 it is seen that at a distance of 4 mm. from the cold plate in air, the

recorded temperature is 14°C. above the line of uniform temperature distribution, whilst in carbon dioxide it is 20°C. Thus if allowance were made for the infinitely thin couple the gradient would still be less steep in carbon dioxide than in air.

Lamp-Black Surfaces.—The horizontal plates were next covered with lamp-black and experiments carried out as before. It was thought that if these difference effects were due to radiation there should be a considerable change in the temperature



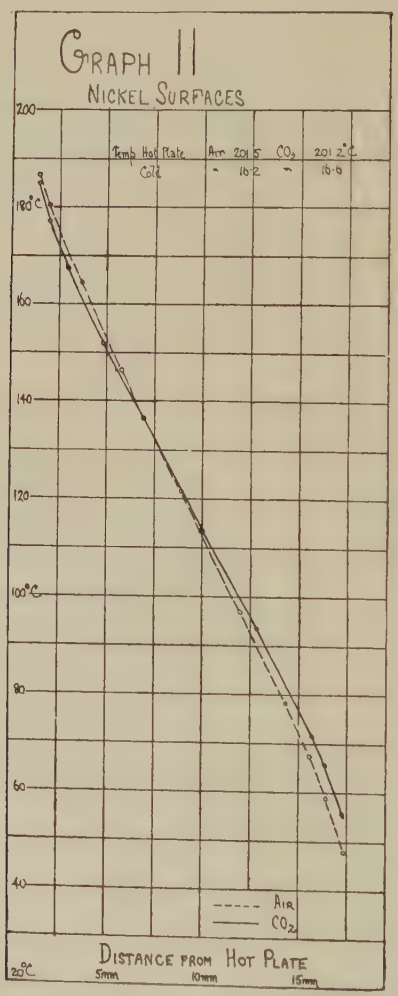
distribution on comparing curves with the highly polished nickel surfaces and with the dull black surfaces.

Graph 12 gives the results with air for bright nickel surfaces and Graph 13 with black surfaces, the plates being about the same temperatures in the two experiments. Graphs 12A and 13A show the corresponding differences. The curve of temperature distribution crosses the straight line of uniform gradient nearer the hot plate with the black surfaces, whilst in the colder portion of the chamber the temperature,

at say Q, with the black surfaces is considerably higher than with the bright surfaces.

Using the black surfaces, Graphs 14 and 14A refer to experiments with hydrogen between temperatures of 206·4° and 23·5°C. and Graphs 15 and 15A to experiments with carbon dioxide between temperatures of 205·5° and 20·7°C. It is seen that the differences with carbon dioxide are about four times greater than with hydrogen.

On comparing 13 and 15 where experiments are carried out in air and carbon

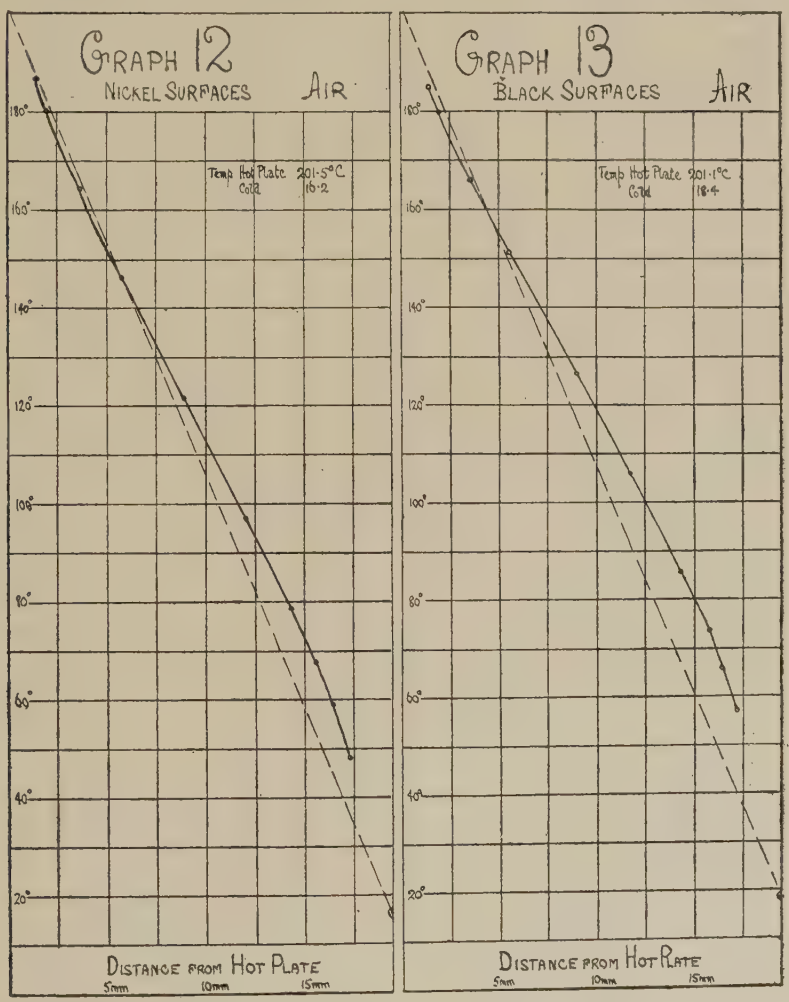


dioxide with black surfaces, it will be seen that the temperature distribution curve in carbon dioxide as measured with the (α) couple is less steep than in air as was seen to be the case with nickel surfaces (Graphs 9 and 10), whilst a comparison of 13A and 15A shows that for the black surfaces the slope of differences is slightly greater in carbon dioxide than in air as was pointed out for nickel surfaces (Graphs 9A and 10A).

Discussion of Results.

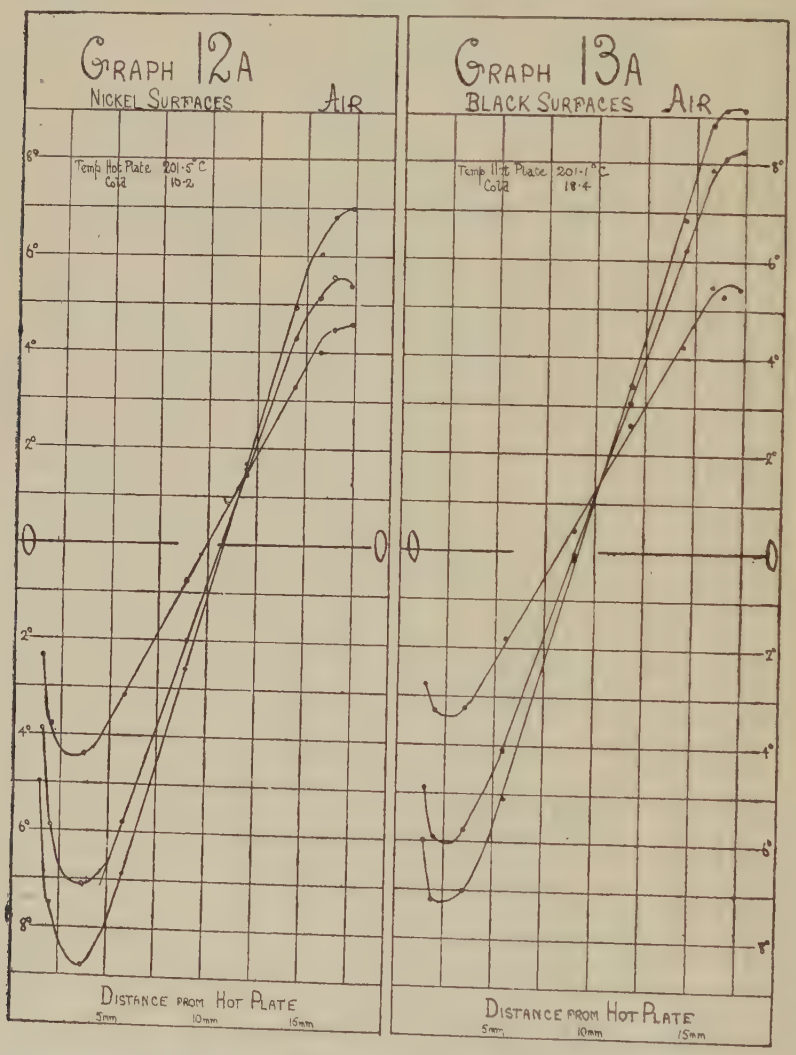
It remains to consider the process by means of which the couples take up their temperatures and to find some explanation of the influence of the diameter of the wires. The factors that must be taken into account are probably limited to :—

(1) The interchange of energy between the gas molecules and the couples,
(2) the radiation received and emitted by the couples,
(3) the residual gas layers condensed on the couples,
(4) the conduction of heat along the wires from the actual junctions.



With regard to No. (4) it has been already shown that precautions were taken in designing the horizontal platform so as to eliminate this effect as far as possible by having considerable lengths of the wires in an isothermal layer, and it is probable that any effect due to conduction is practically negligible.

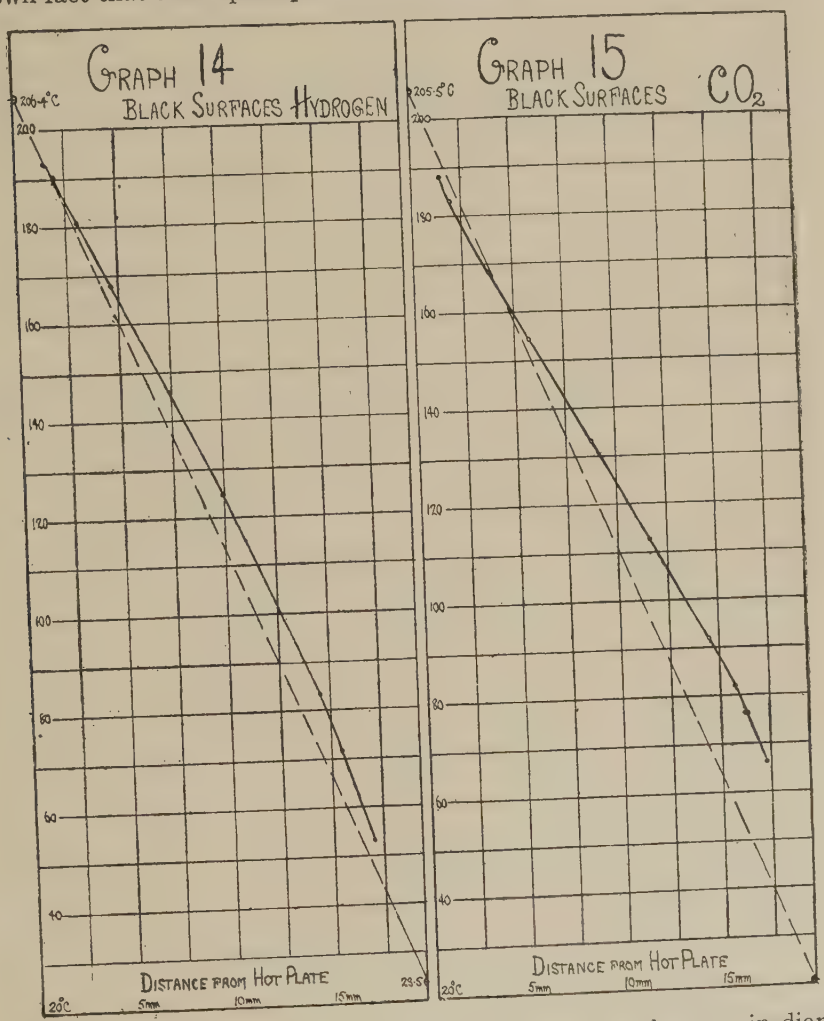
The subject of adsorbed gas layers is one of extreme difficulty, and there is considerable difference of opinion regarding two rival theories, one, that the layer may be several molecules thick, and the other, that it is monomolecular. Experiments have been carried out for instance, showing that charcoal can adsorb many times its own volume of gases at ordinary temperatures and pressures. Other



workers have shown that gases like carbon dioxide and ammonia form a film several molecules thick when brought in contact with glass fibres that have been previously heated to a high temperature at low pressures. Considerable volumes of gases can be detached from the glass walls of a vessel when heated at very low pressures, and estimations have been made that the layer may be as many as 20 molecules thick. Other experiments have been devised in support of this theory showing that the

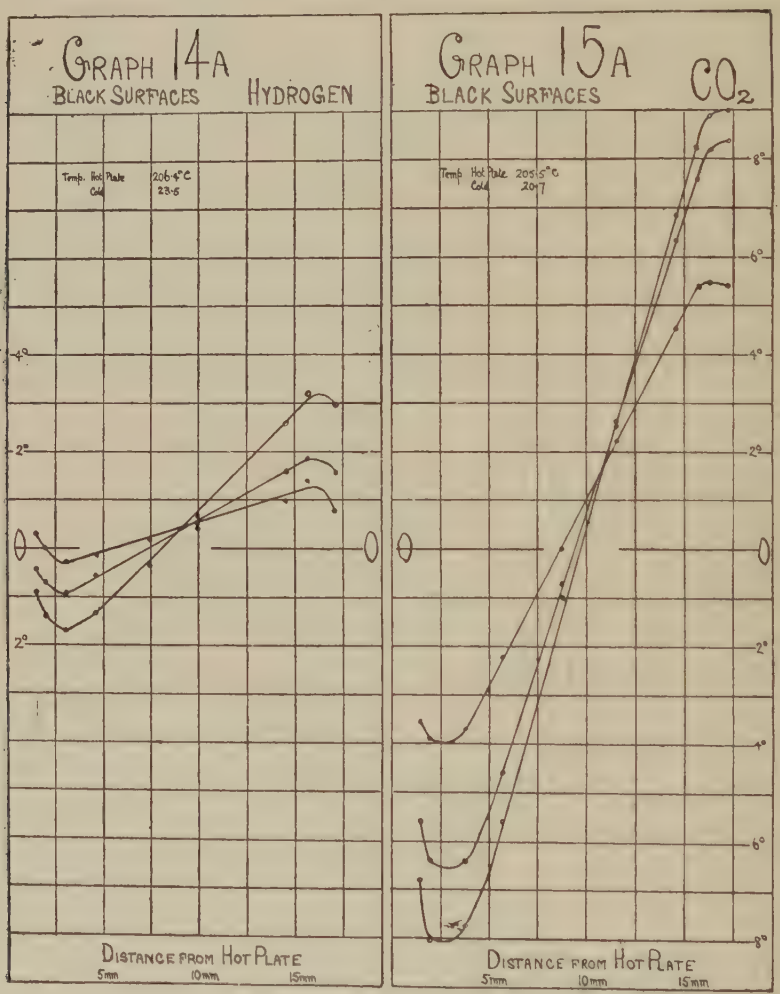
range of molecular forces may extend to a distance equal to a considerable number of molecular diameters.

On the other hand, a large amount of work has been carried out by many workers and especially Langmuir, which gives very strong support to the theory of an adsorbed layer one molecule thick. They also point out that the large amount of gas adsorbed by charcoal depends upon a capillary condensation theory, due to the known fact that the vapour pressure of a liquid inside a fine capillary, the walls



of which are wet by the liquid, decreases greatly with the decrease in diameter of the capillary. With regard to the adsorbed film on glass fibres, they point out that if the gases be very carefully dried, the adsorbed film is only monomolecular and suggest that all the gases used like ammonia and carbon dioxide, being very soluble, are merely dissolved in a film of water on the glass. It must be admitted that most of the evidence seems to be in favour of the single layer. If, however, there be

comparatively thick layers it seems probable that the thickness of the layer would depend upon the diameter of the wires, and it would then be possible to suggest an explanation of the results by giving special physical properties to the layer—a variation in thermal conductivity for instance—different from those of the gas. The thickness of the layer required, however, would be considerably greater than several molecules, so that it would seem to be futile to suggest any explanation based on such a supposition which is not supported by experimental evidence.



With regard to No. (2), radiation seems to play an important part in the experiments described in this Paper. Perhaps the best evidence is obtained from a comparison of Graphs 12 and 13, where experiments are carried out with the *same* gas, air, but with surfaces of highly-polished nickel and of lamp-black respectively. It follows that the quantities of radiation from the two surfaces in the two cases must be quite different. It is seen that in the experiment with the black surfaces

the recorded temperature distribution curve crosses the straight line of uniform temperature distribution nearer the hot plate than in the case of the experiment with the nickel surfaces, whilst nearer the cold plate, as has been already pointed out, the temperature distribution curve is about 6°C. higher with the black surfaces than with the nickel surfaces.

The amounts of radiation received and emitted by a couple for different positions in the chamber cannot be determined accurately. Stefan's formula is not applicable, especially with the polished nickel or brass surfaces. Still, it is instructive to make a *rough* calculation of these amounts of radiation by supposing that the fourth power law can be used. Consider the case with the lamp-black surfaces, the couples also having practically similar surfaces. Let Fig. 2 represent a central section of the

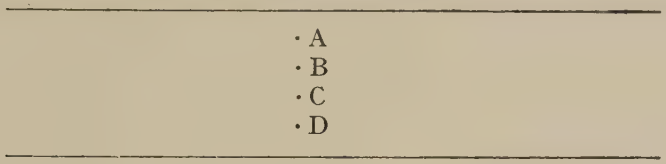


FIG. 2.

experimental chamber. Suppose as in some of the experiments described in this Paper the absolute temperature of the hot plate is 470, and for convenience of calculation let the temperature of the cold plate be 270. Divide the distance between the two plates into five equal parts, and let them be represented by the four points *A*, *B*, *C*, *D*. Then, if the points be on the straight line of uniform temperature distribution, their absolute temperatures would be 430, 390, 350 and 310 respectively. Then, applying Stefan's Law, it can be shown that the ratio of the amount received to the amount emitted in the four cases is 1 : 1·26, 1 : 0·85, 1 : 0·55 and 1 : 0·37. Even allowing that the assumptions made in the calculation may result in a considerable error, it is seen that near the hot plate a couple receives less radiation than it emits, while at the points *B*, *C*, *D* it receives a gradually increasing excess of radiation on approaching the cold plate.

On referring to all the graphs of differences we see a maximum effect a few millimetres from the hot plate. On proceeding towards the cold plate, a point is reached where the lines intersect, suggesting that here the amounts of radiation received and emitted are the same, whilst still nearer the cold plate an excess of radiation is received, and the recorded differences of temperature gradually increase towards the plate The same applies to Graphs 1, 9, 10, 12, 13, 15, all of which show a " dip " near the hot plate, and the gradual slope upwards above the straight line on going towards the cold plate.

With the same assumptions as before, a rough calculation can be made of the distance from the hot plate where the amounts received and emitted by a couple are the same. Taking the figures of the last calculation, the point would be at a distance of rather more than one-fourth of the distance between the plates from the hot plate, and Graph 13 shows that the curve of temperature distribution crosses the straight line at about this distance. The best test, however, is to calculate the position of this point for different temperature gradients between the plates, and it is found that the point approaches the cold plate very slowly at first with decrease of temperature gradient, but much more rapidly when the gradients are small.

An examination of a large number of temperature distribution curves for different gradients fully confirmed this view.

Thus it is evident that radiation plays an important part in determining the temperatures recorded by the couples, but that it is not the *sole* cause of the temperature differences is shown by employing *different* gases in carrying out experiments under exactly identical conditions. Graphs 14 and 15, where the gases hydrogen and carbon dioxide respectively are used, show entirely different results, although the amounts of radiation must be the same in the two cases. The graphs of differences (14A and 15A) for the same experiments show that the temperature differences are about four times as great in carbon dioxide as in hydrogen. It is evident, therefore, that the nature of the gas is of considerable importance, and it is advisable at this point to consider briefly some of the theories relating to molecular impact and the interchange of energy between gas molecules and a solid with which they collide. Nearly all these theories have been suggested to account for the fact that certain experiments on the conduction of heat indicate that there is a poor interchange of energy when molecules collide with walls which are at a higher or lower temperature. One theory claims that molecules of all kinds show little or no tendency to be regularly reflected from a solid surface; that, instead, they condense and re-evaporate in a direction independent of the direction of incidence. On decreasing the temperature, the mean kinetic energy of the molecules becomes less and they evaporate less readily owing to the restraint imposed upon them by the force of molecular attraction. So long, however, as the rate of evaporation is greater than the rate of condensation the molecules of the first layer do not remain long enough to become covered by the others.

Another theory partly supports this view, but claims that a fraction of the molecules condense and re-evaporate, but the remainder are regularly reflected. One theory which has been suggested takes into account the attractive and repulsive forces existing between the molecules of the solid and the gas. A gas molecule approaching a hot plate may strike a plate molecule and be sent back into the gas with an energy greater than its previous energy, but less than the energy equivalent to the temperature of the plate, whilst other molecules, colliding several times with the surface before being able to escape, take up a larger store of energy, part of which is lost, however, when the molecules do work in escaping against attractive forces existing at the surface of the plate.

Experiments illustrating this imperfect interchange of energy have been carried out by Knudsen,* and by an entirely different method by Soddy and Berry.† The latter, in an experiment on thermal conductivity with gases at low pressures, found the loss in watts from a heated platinum strip of known area, which was at a temperature of about 40°C. higher than a cylindrical enclosure kept at constant temperature. From the dimensions of the apparatus, the experimental value of the thermal conductivity could be determined. The theoretical value could also be found knowing the number of impacts and the amount of heat required to raise one molecule through 1°C., and assuming perfect interchange of energy on impact. The ratio of the two gives a coefficient analogous to Smoluchowski's "Coefficient of Thermal Accommodation." Using bright platinum surfaces, Soddy and Berry found that its values for hydrogen were 0·15 at 100°C. and 0·25 at −185°C.; for helium the

* Ann. der Phy., 34, 593 (1911).
† Proc. Roy. Soc., November (1910).

values were about twice as great, while larger values were found for carbon dioxide and nitrogen. It appears to be fairly well established, therefore, that the transference of heat from a solid by molecular impact takes place comparatively slowly, and varies with the gas.

There can be no doubt from the evidence produced so far that the temperature differences are due to two main factors—radiation, and the nature of the gas. An explanation has already been given explaining the effects of radiation in different parts of the chamber, but it is now necessary to suggest why the four couples in the same horizontal plane record different temperatures, and, furthermore, why the thickest couple is lowest near the hot plate and highest near the cold plate.

Consider a couple at the point A in Fig. 2, where it radiates more energy than it receives as radiation. It would, therefore, be lower in temperature than if radiation were negligible. The gas molecules, however, near the point A would not be lowered perceptibly in temperature, since gases are practically transparent to radiation. Therefore the couple would be at a temperature lower than the surrounding gas, so that energy would be transferred by impact in an endeavour to equalise the temperatures.

Consider now the relative *effects* of the two factors. The nett loss of energy by radiation for each couple would differ in amount proportional to its radiating surface. Let the amount be represented by Kr, where r is the radius. They would gain energy by molecular impact, again proportional to their surfaces and equal to $K'r$, K' being a constant for the given plane considered, since the condensation and evaporation takes place at the same rate for the same temperature, but would vary in different planes. There would, therefore, be a nett loss of energy for each couple proportional to $(K-K')r$—that is, the thicker the couple at the point considered the lower would be its temperature. In the same way at, say, the point D, near the cold plate, the couples receive an excess of radiation ($K''r$), and would be higher in temperature than the surrounding gas. They would therefore lose energy by molecular impact ($K'''r$). The gain still being $(K''-K''')r$ for each couple, the thickest couple would now give the highest temperature, a result which agrees with experiment.

Further support is given to this view by Graph 8, which shows that for the three thinnest couples their differences in temperature from the infinitely thin couple vary almost as the radii of the wires.

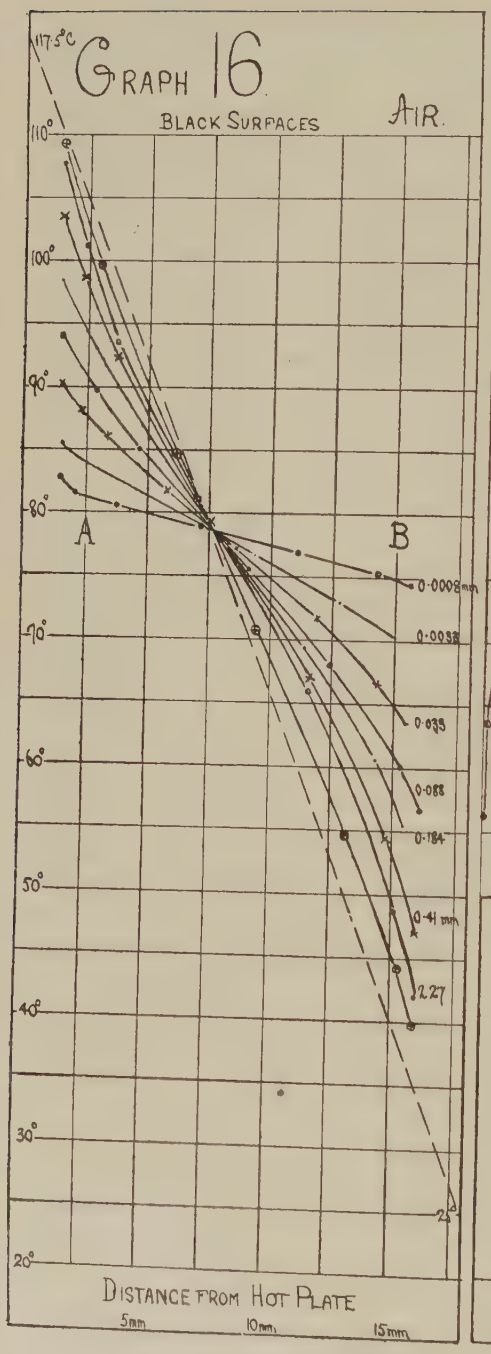
Since experimental evidence has shown that different gases have different "Accommodation Coefficients," we may look to this fact for an explanation of the difference in the results obtained with different gases.

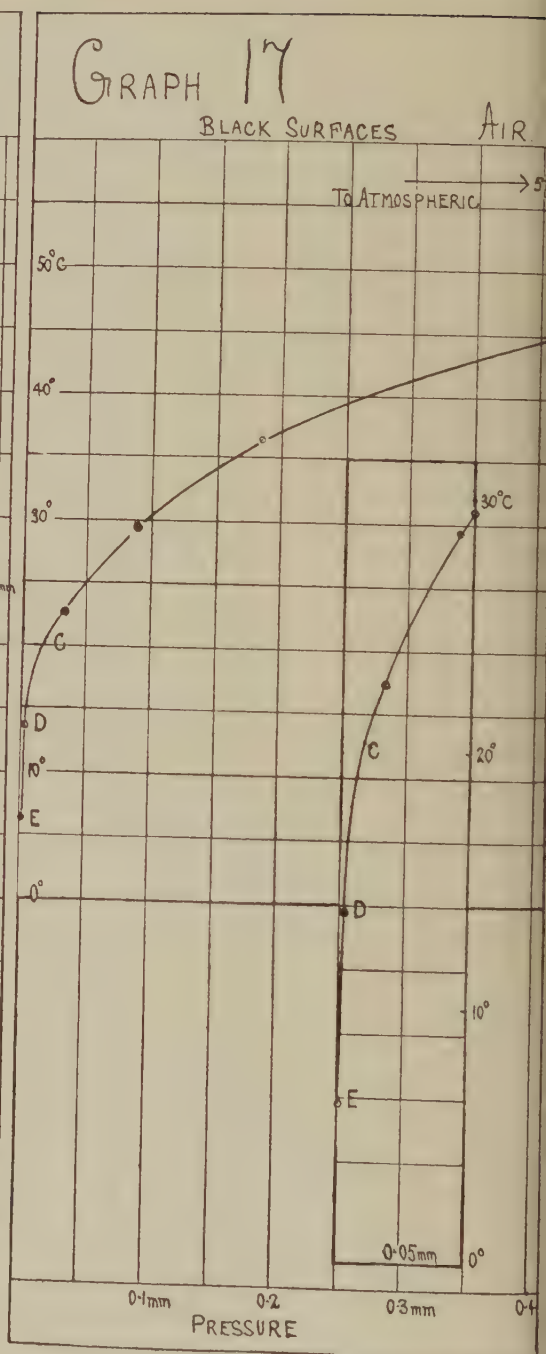
The experimental results and deductions of this Paper seem to have a wide application to measurements of temperature in industry. They suggest that in the measurement of temperature, thermocouples, mercury-in-glass thermometers, and resistance thermometers cannot be relied upon as instruments of precision unless they are used under conditions which are identical with the black-body enclosure.

In conclusion, the author wishes to express his thanks to Prof. Gwilym Owen for his kind interest and encouragement.

Note Added October 31, 1925.

On continuing the experiments in air at low pressures a new set of couples was used, but results are given for the new α couple only. Graph 16 shows the results


GRAPH 16
BLACK SURFACES
AIR.
117·5°C
110°
100°
90°
80°
70°
60°
50°
40°
30°
20°
A
B
0·0008mm
0·0038
0·038
0·088
0·184
0·41mm
2·27
2·5
DISTANCE FROM HOT PLATE
5mm
10mm
15mm


GRAPH 17
BLACK SURFACES
AIR
TO ATMOSPHERIC
50°C
40°
30°
30°C
C
D
10°
E
0°
C
20°
D
10°
E
0·05mm
0°
0·1mm
0·2
0·3mm
PRESSURE

obtained at various pressures, the temperatures of the hot and cold plates being kept constant. These results give strong confirmatory evidence for the correctness of the explanation given for gases at atmospheric pressure. All the graphs pass through a point where the amounts of radiation received and emitted by the couple are equal. It has been shown that between this point and the hot plate there is a nett loss of energy due to radiation alone, whilst between the point and the cold plate there is a nett gain.

It follows immediately that the lower the pressure the less will be the energy transferred by molecular impact, so that in front of the hot plate the couple falls in temperature until a state of equilibrium is reached which will depend upon the pressure. It follows also that these two surface effects will depend upon the thickness of the couple, so that by using a single couple we immediately deduce that thicker couples must read lower temperatures near the hot plate and higher temperatures near the cold plate, and that these readings would vary with the nature of the gas.

An interesting point regarding the energy transferred by molecular impact is shown on taking two fixed points A and B in the gas, and finding the temperature difference between them at different pressures.

At atmospheric pressure this is 57·0°C., and for the several pressures 50·6, 44·8, 36·6, 29·5, 22·7, 13·7, and 6·3°. These temperature differences are plotted against the pressures in Graph 17.

It is seen that the temperature difference between the points falls slowly and fairly uniformly from atmospheric pressure to a pressure represented by the point C, where the mean free path is about 0·3 mm. At lower pressures the rate of fall is much greater, and at the point D the mean free path is approximately equal to the distance between the plates when the curve becomes considerably steeper.

This can be explained as follows: When energy is transferred by molecular impact the colliding molecule immediately receives energy from the gas molecules in the vicinity—that is, the conductivity of the gas comes into play. On reducing the pressure until the mean free path of the molecules is of the same order as the dimensions of the chamber, conduction ceases, and we have then simply a molecular flow between the plates, the molecules receiving energy directly from the walls, and this state would be represented by the steep portion of the curve at the point D. On still further reducing the pressure, the fall in temperature would vary directly as the number of molecules, so that we would expect the portion of the curve DE to be a straight line, as indicated in the graph.

DISCUSSION.

Mr. R. S. Whipple said that as the readings of the thermocouples were so much affected by the nature of the gas in which they lay and so little by the change of surface from black to bright he could not help feeling that the author had under-rated the effect of gaseous conduction as compared with radiation. Would it not be well to construct the thermocouple leads of horizontal stretched wires so that they might be made to coincide with isothermal layers of gas?

Mr. C. R. Darling congratulated the author on the very large number of observations he had taken, but said that in spite of this he felt that the conclusions which had been reached needed confirmation: convection must surely play a considerable part in such phenomena. The Paper constituted a warning, however, against the incautious use of thermocouples. The method of interpolating to zero attributed to Kreisinger and Barkley was, he believed, first put forward by Smithells in connexion with his work on gas films.

Dr. J. S. G. Thomas said that the large amount of patient work described in the Paper is of great interest to industries in which thermocouples are much employed. If he could fully

accept the author's low estimate of the reliability of thermocouples he should feel considerable uneasiness, but he was inclined to think that the effect of convection had been under-estimated: convection currents are excessively difficult to eliminate, as was found in such work as that of Boys on the gravitation constant, and in industrial measurements large allowances have to be made for them. The author's results suggest that in some cases there may be errors amounting to 50 per cent. of the actual temperature, but there is no reason to believe that in industry such errors as that occur, in view of the precautions taken. An excellent study of the effects of conduction has been given by Daynes in the account of his katharometer, and he believed this might throw light on the results obtained by the author. As regards the method of interpolation mentioned by Mr. Darling, he rather thought that Nichols and Hull were before Smithells in that matter.

Dr. Geoffrey Martin said that though not himself a physicist he had to use thermocouples in attempting to balance the input and output of heat in a cement-making process, and he would be very glad of advice as to the cause of gross errors which he had encountered.

Mr. Guild pointed out that the readings of the thermocouples depended on the properties of two metals. The problem might be simplified by using a single metal in the form of a resistance thermometer.

Mr. J. H. Brinkworth said that even if a platinum resistance thermometer were used, the readings would depend on the nature of the gas. He felt a personal interest in the subject of the Paper because it had a bearing on some measurements of the ratio of the specific heats of hydrogen made by Mr. Partington and by himself, the former's values being higher than his own. In these experiments the temperature was measured with a platinum resistance thermometer and a correction had to be made for the conductive effect of the gas on the platinum, this effect having been standardized by measurements made in air. In fact, however, the correction applicable in hydrogen would be smaller than that applicable in air, and taking the author's figures it appeared that Mr. Partington's results should accordingly be reduced by approximately the amount which would make them agree with his own.

Mr. R. S. Troop (communicated): The author of this Paper is to be congratulated on some very useful, interesting and well-planned experiments dealing with a subject whose application in industry is rapidly growing. There are many occurrences in industry of "unequally heated enclosures" where accurate temperature measurements are required, and the Paper indicates some precautions necessary to attain this accuracy when thermocouples are used, and the difficulty of obtaining "instruments of precision" under these conditions. The results also show that due care must be taken in making deductions from thermocouple readings and allowances made for the effects recorded in the Paper before too great stress is laid on small peculiarities in temperature readings. The work on the effect of atmosphere on temperature readings will have particular interest for those dealing with kilns, ovens, flues, etc. It is good to note that Mr. Mandell promises us further work on the subject. The question of the effect of the materials of the thermocouple is not dealt with in the Paper, and I should like to ask whether this has been considered by the author. At higher temperatures this may be important, in view of Sir G. Beilby's work on the effect of atmosphere on the recrystallisation of metals, their surface adsorption, etc. One would like to know also how atmospheric differences would affect the e.m.f.s. of platinum-platinum-rhodium and of nickel-chrome couples which are of considerable industrial importance.

Author's reply (communicated): With regard to the references in the discussion that the effects of convection may be considerable, it is necessary to point out that we have, in effect, a layer of gas about 2 cm. thick bounded by a large, hot upper plate and a lower cold one some 12 cm. in diameter, so that there would presumably be little tendency for the circulation of convection currents in the gas. It is difficult to see how a more ideal arrangement could be designed.

The remarks of Mr. Guild and Mr. Troop contain suggestions which would undoubtedly be of value in the event of further work being done on the subject.

Three references are made in the discussion to the effect of the conductivity of the gas. A note added to the present Paper is in effect a reply to that criticism, as it shows the intimate relations between the transference of energy to the couples by molecular impact, and the conductivity of the gas.

VII.—PHOTO-ELECTRIC RADIATION PYROMETER.

By Prof. F. A. Lindemann, F.R.S., and T. C. Keeley.

Read at Oxford, July 4, 1925.

Abstract.

By means of two photo-electric cells with suitable coloured filters the energy radiated in two distinct spectral bands is compared. By combining the ascertained sensitivities of the two cells and their filters with the radiation law the black-body temperature of the radiator can be determined.

SOME years ago one of the authors suggested that by measuring the photo-electric currents produced by the same source of light on two metals sensitive to different parts of the spectrum it should be possible to obtain an estimate of the temperature of the source of light.* Although the original suggestion was directed particularly to the determination of stellar temperatures, the method is obviously suitable for terrestrial sources. It may be of interest to give a preliminary account of the instrument designed for this purpose, more especially since a pyrometer based on the same principle made in the General Electric Company's laboratories has recently been described.†

The construction is shown in Fig. 1. By means of the lens A the light from the source is focused on the fixed diaphragm D, so that the light enters the apparatus always in the same direction. The lens B renders the light parallel, and it is then partly transmitted, partly reflected, by the half platinised mirror M. The two beams are focused by the lenses C and D on to the optical wedges W_R and W_K, and after passing through two coloured filters F_R and F_K, they enter the photo-electric cells R and K, which contain rubidium and potassium respectively. The cells are connected in opposition to the needle of the electrometer, so that when the current is the same in each the electrometer shows no deflection. The electrometer needle is connected to earth through a high resistance leak (of the order of 10^{10} ohms).

A source of known temperature is used to fix the scale. The wedge W_K is set at some convenient reading, say d_0, and the coarser wedge W_R is adjusted until the electrometer shows no deflection. Final adjustment is rendered more accurate by means of the shutter S, which is opened and closed by hand; the electrometer should show no flicker as the cells pass from the illuminated to the dark condition. This adjustment made, the wedge W_R is fixed permanently in position.

When the instrument is focused on the source whose temperature it is desired to measure, the wedge W_K is moved until the electrometer shows no deflection—i.e., until the current from the two cells is equal. Suppose its reading is d_1, and that the source is at a higher temperature than the standard, so that d_1 is greater than d_0. Then the ratio in which the light entering the potassium cell must be reduced so that its current balances that from the rubidium cell, is $k \log (d_1 - d_0)$, where k is the "wedge constant." As will be seen, this is a measure of the temperature of the source.

* Monthly Notices, R.A.S., March (1919).

† Journ. Sci. Inst., Vol. 11, No. 6, p. 177, March (1925).

Suppose the currents produced by unit light energy on the potassium and rubidium cell respectively are given by $f_K(\lambda) \cdot \delta\lambda$ and $f_R(\lambda) \cdot \delta\lambda$, and that the ratio transmitted by the two filters are $\varphi_K(\lambda) \cdot \delta\lambda$ and $\varphi_R(\lambda) \cdot \delta\lambda$. Then, for a black body source at a temperature T_0 the currents produced by the two cells will be respectively given by

$$i_K = c_1 \int f_K(\lambda)\varphi_K(\lambda) \cdot e^{-\frac{a}{\lambda T_0}}/\lambda^5 \cdot d\lambda$$

$$i_R = c_2 \int f_R(\lambda)\varphi_R(\lambda) \cdot e^{-\frac{a}{\lambda T_0}}/\lambda^5 \cdot d\lambda$$

where c_1 and c_2 are constants depending on the apparatus.

Integration extends over the wavelength range to which the combined cell

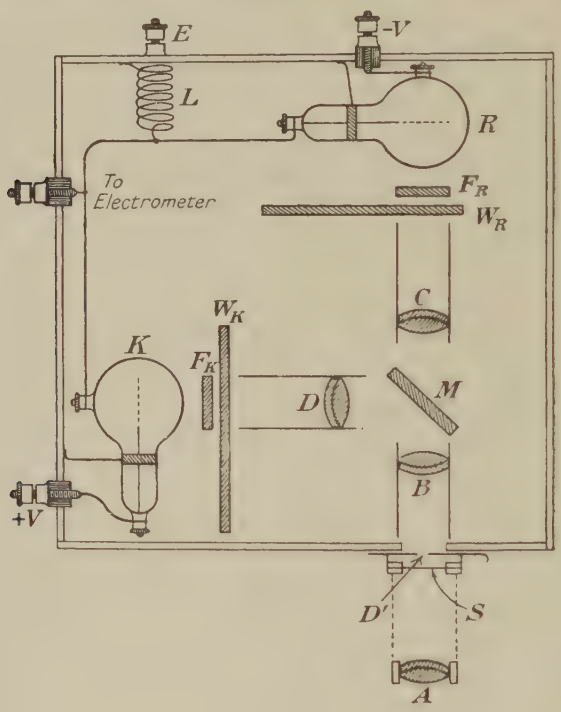


FIG. 1.

and filter system is sensitive, Wien's radiation law being used, since for the wavelengths in question, this differs from Planck's by not more than $\frac{1}{4}$ per cent. even at 5,000. As the cell and corresponding filter are always used together, we can write $f(\lambda) \cdot \varphi(\lambda)$ as $F(\lambda)$, and we have

$$\left.\begin{aligned} i_K &= c_1 \int F_K(\lambda) e^{-\frac{a}{\lambda T_0}}/\lambda^5 \cdot d\lambda \\ i_R &= c_2 \int F_R(\lambda) e^{-\frac{a}{\lambda T_0}}/\lambda^5 \cdot d\lambda \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (1)$$

Suppose now that the pyrometer is focussed on a source of temperature T_1. The currents produced will be

$$\left.\begin{aligned} i'_K &= c_1 \int F_K(\lambda) e^{-\frac{a}{\lambda T_1}} / \lambda^5 \, . \, d\lambda \\ i'_R &= c_2 \int F_R(\lambda) e^{-\frac{a}{\lambda T_1}} / \lambda^5 \, . \, d\lambda \end{aligned}\right\} \quad \ldots\ldots\ldots \quad (2)$$

If the currents in (1) are made to balance by adjustment of the wedge W_R, then

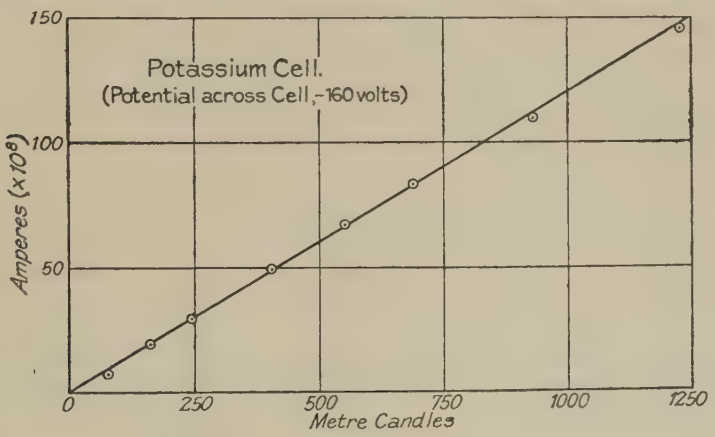


FIG. 2.

the currents in (2) will not balance. The distance the wedge W_K must be moved to bring about a balance will be given by

$$k \log (d_1 - d_0) = (i'_K / i'_R) / (i_K / i_R)$$

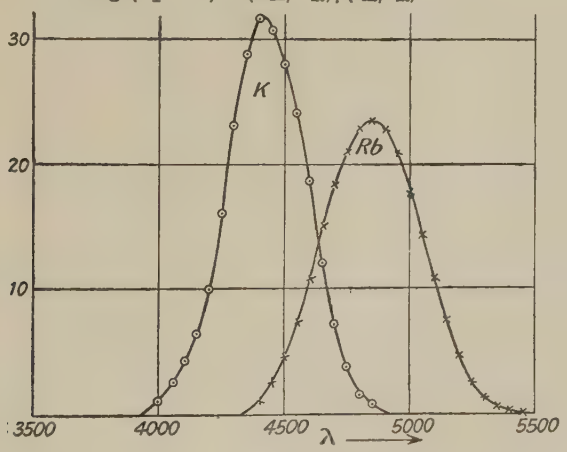


FIG. 3.

We have assumed in deducing the above result that the photo-electric current is proportional to the intensity of the light falling on the cell. This was verified for the type of cell used, as is shown in Fig. 2.

The form of the functions $F_K(\lambda)$ and $F_R(\lambda)$ may be found by illuminating the aperture of the pyrometer with monochromatic light from a constant deviation spectrometer, and measuring the currents from each cell separately. The source of light was a carbon filament lamp, the temperature of the filament being measured by means of an optical pyrometer. The two curves giving the sensitivities per unit energy received are shown in Fig. 3.

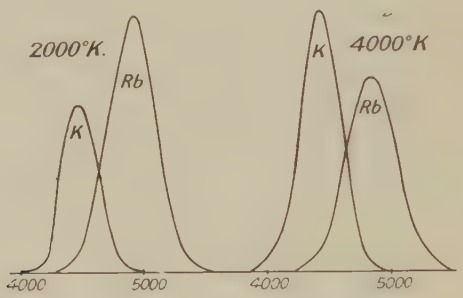


FIG. 4.

The filters used were chosen so as to accentuate the sharpness of the maxima due to the selective photo-electric effect. By combining these curves with the radiation curves at various temperatures from 1,800°K to 5,000°K, measuring the areas and finding their ratios, the readings of wedge W_R are calculated in terms of the tempera-

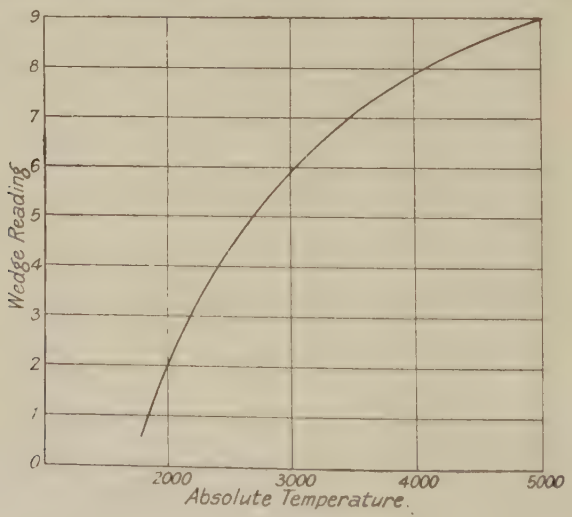


FIG. 5.

ure. Fig. 4 shows the respective values of the currents produced in the two cells at 2,000° and 4,000°, the scale of the diagrams being, of course, very different.

Fig. 5, giving the calculated curve, shows the temperature corresponding to any given reading of the wedge used. The wedge constant was found photoelectrically, and agreed with that given by Messrs. Ilford to less than 1 per cent., and was constant

over the whole wedge, the result, of course, confirming the proportionality of light and photoelectric effect.

It will be seen from the last curve that at 2,000° a change of wedge reading of 1 cm. corresponds to about 160° ; at 3,000°, to 300° ; at 4,000°, to 700°. By reading to 1/10 mm. (and this setting can be repeated without any difficulty), the temperature at 2,000°, 3,000° and 4,000° can be found to an accuracy of 1·6°, 3° and 7° respectively. By the use of a finer wedge, temperatures over a more confined range could be measured even more accurately.

DISCUSSION.

Dr. C. Chree : There seems a promising field for an instrument capable of even moderate accuracy at the high temperatures mentioned. The accuracy would presumably depend on the precision with which it measured known temperatures below 2,000°C., such as the melting point of platinum. It is a subject on which we look for enlightenment to the National Physical Laboratory.

Mr. F. E. Smith : The author has shown the need for someone—preferably the Heat Department of the National Physical Laboratory—to discover a simple means of accurately reproducing a high temperature, such as the melting point of platinum, for standardisation purposes.

Mr. J. Guild : I am particularly interested in the measurement of high temperatures. The photo-electric method is one of great promise in view of the high sensitivity. There are, however, a number of difficulties involved in the use of a method depending on the relative indications of two separate cells. Some of these were referred to in a Paper by N. R. Campbell and H. W. B. Gardiner, published in the Journal of Scentific Instruments for March, 1925, which contains an account of preliminary work on similar lines. I should be interested to hear if Prof. Lindemann has experienced any trouble due to inconstancy of the cells, variations in the " dark current," etc. My own feeling about photo-electric cells is that these are not yet instruments of such constancy of behaviour that we can safely determine characteristic curves, and regard these subsequently as instrumental constants in work of high precision. I should on this account prefer a null method in which one cell only is used. If filters be introduced to give the difference in wavelength sensitivity, there seems little point left in employing two cells. It is important to remember that measurements of this general type determine what is known as " colour temperature," as distinct from " brightness temperature," which is measured by optical pyrometers on a photometric basis. These temperatures differ from one another and from true temperature on the work scale, except in the case of radiators, which give full " black body " radiation. In the general case the scales are not directly comparable with one another without a complete knowledge of the emissive properties of the surface of the radiating material.

Dr. G. W. C. Kaye (communicated) : The several advantages, and not the least the relative simplicity of the photo-electric type of radiation pyrometer, which Prof. Lindemann describes, should lead to its extended use, at any rate for research purposes. The instrument will be a welcome addition to the other types of pyrometers in existence for exploring the region of really high temperatures. Prof. Lindemann's desire for a " jumping-off " temperature standard at about 2,000°C., which can be easily reproduced, is one that the N.P.L. will naturally bear in mind.

Authors' reply (communicated) : We have always found our cells reliable and constant, and no difficulty has been experienced either with dark current or with a variation of sensitivity provided a luminous discharge is avoided. Of course, if we adjust the scale W_K to give true readings of the temperature for a black body, we measure only the colour temperature for a coloured body. If, however, we adjust the wedge W_R so that at some known temperature for a given coloured body, the wedge W_K reads this temperature, it will continue to indicate the true temperature for that body (with the same scale used for the black body), provided that the regions of the spectrum used are sufficiently narrow.

VIII.—MEASUREMENT OF THE AMOUNT OF OZONE IN THE EARTH'S ATMOSPHERE.

By G. M. B. Dobson, *D.Sc.*, and D. N. Harrison, *B.A.*

Read at Oxford, July 4, 1925.

Abstract.

The quantity of ozone present in the atmosphere has been measured by spectroscopic means on about a hundred days. A marked connexion with the pressure distribution at the surface, and particularly with the conditions at about 10 km., has been found.

OZONE has two strong absorption bands, one in the ultra-violet and one in the infra-red. It therefore absorbs some of the sun's radiation in addition to the earth's radiation. The ultra-violet absorption does not give appreciably increased radiative power, because the ultra-violet radiation of a black body at the temperature of the atmosphere is negligibly small. The equilibrium temperature of ozone is therefore higher than that of water-vapour or CO_2, which only absorb in the infra-red. It thus seems likely that variations in the amount of ozone might affect the temperature of the atmosphere in its neighbourhood—i.e., at great heights—and consequently the pressure at the earth's surface. It has been shown that there is no appreciable quantity of ozone near the surface, and all that there is, is probably above the troposphere.

The method used for measuring the ozone is a modification of that due to Fabry and Buisson. The ultra-violet absorption band of ozone begins at about 3,300 Å., and rapidly becomes stronger for shorter wavelengths, cutting off the sun's spectrum completely at about 2,850 Å. This region of the spectrum is photographed, and the intensities of a number of the Fraunhofer transmission lines are measured. Thus the difference in the absorption of any two wavelengths by the atmosphere is deduced. Before we can obtain the absorption coefficients we have to know the intensities of different wavelengths outside the atmosphere—i.e., before absorption; these are found by plotting the intensities as measured on the earth at different times of day against the distance through the atmosphere travelled by the rays, and extrapolating the curves to the point corresponding to zero distance. The difference of absorption of any two wavelengths is due partly to the different amount of scattering by air molecules and partly to the different absorption by ozone; the former effect can be allowed for, and hence the amount of ozone necessary to produce the remaining difference of absorption can be calculated. This may be expressed as the thickness which the ozone actually present would have if reduced to a layer of pure ozone at N.T.P., and the average value is about 3 mms.

Photographs are taken with a Féry spectograph, which has a special filter of bromine and chlorine in front of the slit to cut off all wavelengths longer than 3,300 Å., which would otherwise cause too much scattered light in the instrument. An optical wedge is placed immediately before the plate. Fig. 1 shows two photographs taken at about the same altitude of the sun, one on a day when there was little ozone, and the other on a day when there was a comparatively large amount.

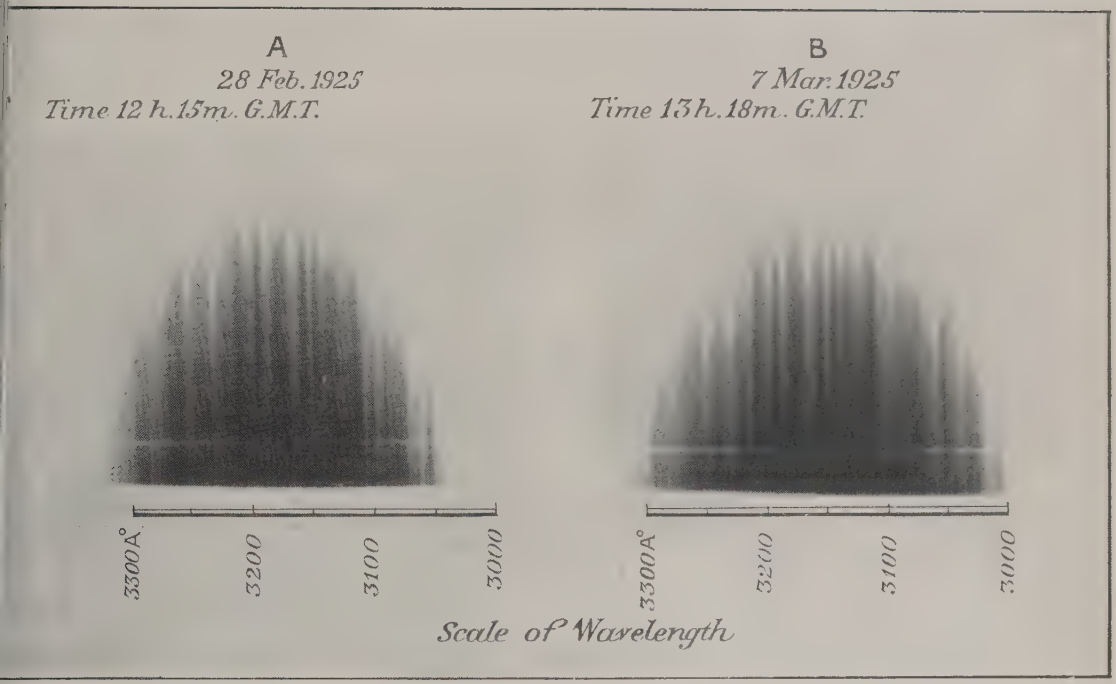


FIG. 1.

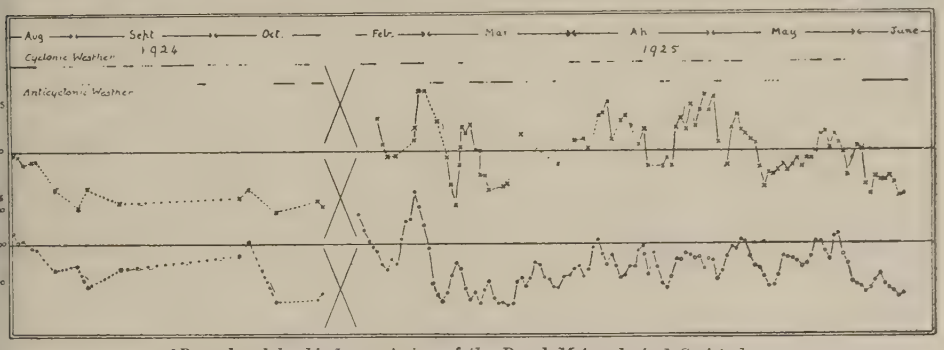
To face page 74]

he heights of the lines are a measure of the intensities. It will be seen that the long avelengths are about equal in the two cases, since there is practically no absorption y ozone. At the short wavelength end one photograph is much weaker than the her, owing to the absorption by the larger amount of ozone.

Fig. 2 shows the results of measurements, using the lines 3,232 Å. and 3,052 Å. he upper curve shows the amounts of ozone, in arbitrary units, on each day when bservations were possible. The lower curve represents the daily values of the baro- etric pressure at Oxford (plotted inversely), and it is obvious that there is a correla- on. The series of horizontal lines above refer to weather conditions ; the upper ones ow the occurrence of cyclones ; and the lower, anti-cyclones. Dots mean that the nditions were not well marked. It will be seen that the amount of ozone tends be low during an anti-cyclone and high during a cyclone, and that the maximum d minimum differ by about 50 per cent.

Other curves have been obtained, using other pairs of wavelengths, and these gree entirely with the one shown.

The sign of the correlation is that which would be expected, as an increase in he amount of ozone would tend to produce a higher temperature in the upper air,



[*Reproduced by kind permission of the Royal Meteorological Society.*]

FIG. 2.

hus reducing the mass of air above the surface, and consequently the surface ressure.

It is further of great interest that the correlations with the pressure are uch closer at great heights than at the surface. A fuller account has been ublished in the Quarterly Journal of the Royal Meteorological Society.

DISCUSSION.

Dr. C. CHREE : The subject of the Paper is of much interest to meteorologists. One thing e should like to know is the level where the ozone is found, whether at the top of the troposphere r at a higher level. It would be of much interest to have a comparison of corresponding daily alues of ozone and of the " solar constant " as measured by Dr. Abbot and his collaborators.

Prof. W. F. G. SWANN : I should like to inquire as to the altitude, or rather the pressure, f the regions of the atmosphere to which the Paper refers. I am interested in this because think there is some reason for supposing that the absorption of radiation by a gas at low pressure ay be smaller than might be supposed at first sight. Presumably the energy becomes absorbed n the first instance in intermolecular transitions—the types of transitions associated with band pectra—and is only transferred to kinetic energy of translation at impact. The mean free time etween impacts is of the order 3×10^{-10} seconds at atmospheric pressure. If the pressure were educed to one hundredth of an atmosphere the time would be of the order 3×10^{-8} seconds. I

is a matter of some interest to inquire whether a molecule could hold its energy for this length of time without falling back to its normal state through radiation. If it could not, then, at such pressures, many of the molecules would fail to convert the energy which they had absorbed into translation of kinetic energy, and would re-radiate it, so that to this extent the heating of the gas would be reduced. What the net result of this process would be is a matter for some speculation, but the problem of the effect of low pressures on heat absorption in gases is one not without interest. I presume, however, that the pressures to which the Paper refers are much higher than those which would be concerned in the phenomenon I have discussed.

Mr. L. F. Richardson: This interesting application of laboratory method to the upper air shows that the pressure is correlated with the ozone content—a valuable new fact. It is, however, difficult to accept the authors' suggestion that the heating of the ozone may be the cause of the pressure change, for the ozone is not well placed for producing mechanical disturbances, being too high up. In the domestic hot water system the boiler must be placed low down. Sandström has called attention to the importance for meteorology of the level of heating, and recently H. Jeffreys (in a Paper read at the Royal Meteorol. Soc., May, 1925) has adjusted "Sandström's Theorem" in detail without, I think, diminishing its importance. So before accepting the authors' suggestion, which is so different from the Bjerknes view of cyclones, one would like to see a numerical calculation of the dynamical effects of heating the ozone. A more likely explanation of the correlation between ozone and pressure is that large masses of air have moved to Oxford from various latitudes; that both pressure and ozone-content have values characteristic of the radiation (waves or particles) of the latitude in which the air has recently wandered; and that during the journey to Oxford these characteristics, though changing, have not usually changed beyond recognition. This explanation is in accord with Goldie's investigation of temperatures and trajectories (A. H. R. Goldie, Quart. Journ. R. Meteor. Soc., January, 1923, p. 6).

Prof. A. Fowler: I have been very much interested in this Paper. I recall that when Lord Rayleigh and I were investigating the absorption bands of atmospheric ozone in the spectra of the sun and stars some years ago, we were inclined to think that it might be possible to study the variations by comparing the intensities of the narrow absorption bands with those of adjacent groups of Fraunhofer lines. Such an investigation, however, called for more continuous observation than could be conveniently made in connection with college work, and we did not pursue the matter further. The method suggested by Dr. Dobson and Mr. Harrison seems to me very promising, though it was somewhat difficult to recognise the range of wavelengths observed in a hasty glance at the negatives on the screen, and a good deal may depend upon the choice of wavelengths to be compared. I was surprised to find that such large variations had been noted in the ozone absorption. Fabry and Buisson estimated the total amount of ozone ordinarily present to be equivalent to about 3 mm. thickness at atmospheric pressure, and the variations indicated by their observations were comparatively small. One of the most interesting questions connected with the atmospheric ozone is that of a possible variation with the 11-yearly period of the sun, depending upon a corresponding variation in the ultra-violet radiation. If the new observations are correct, it would seem that the direct effect of the solar variation is likely to be submerged by the large variations from day to day arising from other causes. I think that many besides myself will look forward to the opportunity of studying the details of the method and results when the Paper is printed.

IX.—THE ABSORPTION COEFFICIENT FOR SLOW ELECTRONS IN MERCURY VAPOUR.

By Robert B. Brode, *B.Sc., Ph.D.*, Rhodes Scholar, Oriel College, Oxford.

Read at Oxford, July 4, 1925.

Abstract.

The absorption coefficients of electrons having from 0·5 to 100 volts velocity were observed in the vapours of mercury, cadmium and zinc. Except for a small irregularity in cadmium, the absorption coefficient decreased uniformly with increasing velocity.

A MOLECULE has an effective area, due to its electric field, within which a passing electron will be deflected from its path. The total effective area of all the molecules in a cubic centimetre of gas at a pressure of 1 mm. of Hg is known as the absorption coefficient. If a beam of electrons is sent through a path length x in a gas at the pressure p, then

$$I = I_0 e^{-\alpha x p}$$

where I_0 is the number of electrons in the beam at the beginning of the path, I the number which passed through the gas without being deflected, and α the absorption coefficient. This absorption coefficient has been observed for most of the common gases by Lenard and others.*

Apparatus.

The apparatus (Fig. 1) used in this experiment consisted of a filament F, two

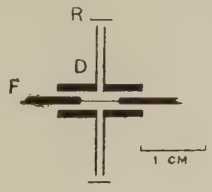


Fig. 1.

parallel discs D, and a ring R. The electrons were accelerated from the filament to the guard cylinders attached to the discs, and some of them continued on (to the ring) at uniform velocity, in the field-free space between the discs. The decrease in the number of electrons arriving at the ring when the mercury vapour was introduced was due to the deflection of those electrons that passed within the effective deflecting area of a molecule.

* O. Lenard, Ann. der Phys., 12, 714 (1903). H. F. Mayer, Ann. der. Phys., 64, 451 (1921). C. Ramsauer, Ann. der Phys., 72, 345 (1923). R. B. Brode, Phys. Rev., 25, 636 (1925).

The bulb containing the apparatus was of Pyrex glass. The metal parts were all made of nickel except the tungsten filament and the tungsten seals through the Pyrex. The apparatus was baked out for four hours at 550°C., and the nickel parts heated to a bright red heat by bombardment with 1,500 volt electrons from the filament. A small amount of mercury was distilled into the bulb, which was then sealed off from the vacuum pumps. After sealing off the bulb the pressure of the vapour could be determined from the temperature of the metal.

Results.

The values of α were observed for electrons with velocities from 0·4 to 150 volts, as shown in Fig. 2. From 50 to 150 volts α decreased uniformly, from 22·5 at 50 volts, and 11·5 at 100 to 4·5 at 150 volts.

Above 100 volts the value of α is in good agreement with the values observed by Nettleton* for the effective area within which an electron must come to produce

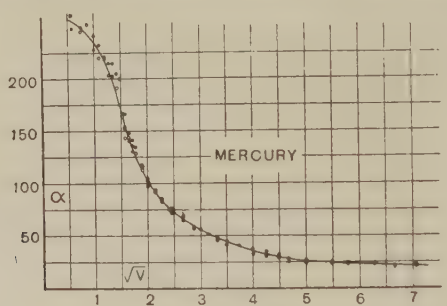


Fig. 2.

ionisation. From this it appears that the deflection of a high velocity electron also results in the ionisation of the molecule. The rate of increase of the effective area becomes less below 3 volts, but no indication was found of the maximum whose existence has been suggested by Minkowski.† The remainder of the curve above 3 volts agrees quite well with the theory given by Zwickey.‡ When the field acting on the electron is only that due to the polarisation which the electron itself causes in the molecule, the value of α varies as K/V^2, where K is a constant depending on the dielectric constant of a single molecule.

Since the reciprocal of the absorption coefficient is the free path of the electron at 1 mm. pressure, it will be seen that the free path is approximately proportional to the velocity. In problems of diffusion, where the velocity of the electron is not constant, the correct results will not be obtained with the assumption of constant free path.

A more complete account of this investigation of the absorption coefficient for mercury, together with observations on cadmium and zinc, will be published in the near future.

* L. I. Nettleton, Proc. Nat. Acad. Sci., 10, 140 (1924).
† R. Minkowski, Zeit. f. Phys., 18, 258 (1923).
‡ F. Zwickey, Phys. Zeit., 24, 171 (1923).

DISCUSSION.

Prof. E. N. DA C. ANDRADE: Mr. Brode's Paper is very interesting, not only as confirming the surprising results obtained by Ramsauer with the inert gases, but as showing that other gases which are not monatomic can exhibit the same unexpected property—namely, that the absorbing cross-section, after increasing with diminishing velocity of the electron, reaches a maximum and then abruptly diminishes. We must therefore look for the explanation of this effect, not in the peculiar closed structure of the extranuclear electrons of the inert gases, but in some more general feature. In this connexion certain differences between Ramsauer's results and those of Mr. Brode are notewothy. Whereas for hydrogen and nitrogen Ramsauer finds that the absorbing cross-section increases as the electron velocity is indefinitely diminished to a constant value, which in the case of the former is a few times the gas-kinetic cross-section, and in the case of the latter very slightly exceeds the gas-kinetic cross-section, Brode finds both for them and for other gases an indefinite increase of cross-section down to the lowest velocities which he could employ. If these results are substantiated, it is possible that for all atoms and molecules there is a maximum, but that in the case of nitrogen, hydrogen and the others it occurs at such low velocities that it could not be demonstrated. Apart from a subsidiary maximum, probably attributable to a critical potential of the atom in that region of potential, Mr. Brode's curve for cadmium vapour(?) also shows an indefinite increase of absorbing cross-section, which again indicates that the difference between the curves for hydrogen and helium, say, is not due to the difference between a diatomic and a monatomic gas. Ramsauer's work has not, in my opinion, received sufficient attention, and I am very glad that it is being extended in this country. The explanation of the maximum is not easy, and, in spite of the attempts of Hund and others, it may be said that it is still to seek. When it comes it will probably throw some light on the problem of atomic structure.

Dr. G. W. C. KAYE: The author has fully realised that for useful measurements to be made it must be possible to eliminate completely all foreign matter and to be able to isolate the tube completely as occasion requires. I am much taken with the very neat device by which the closing stopper can be controlled at will by the heating and expansion of the supporting steel wire.

X.—TWO NEW TYPES OF HIGH VACUUM GAUGE.

By E. Bolton King, Duke of Westminster Research Student of Christ Church, Oxford.

Read at Oxford, July 4, 1925.

Abstract.

Descriptions are given of a new type of decrement gauge in which the individual oscillations are photographed, and a variation of the Pirani gauge, in which no metal is exposed to the vacuum.

THE first gauge is of the decrement type, depending for its action on the variation of viscosity with pressure when the latter is such that the mean free path of

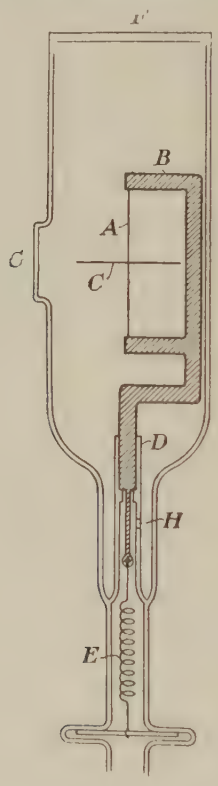


Fig. 1.—Diagram of Decrement Gauge.

the gas molecules is of the same order as the dimensions of the apparatus. The essential part consists of silica fibre A (Fig. 1), between 3 and 4μ in diameter and 4 cms. long, which is fixed by a small amount of gold to the silica frame B, the tension, which thus does not vary with changes in temperature, being about half that required

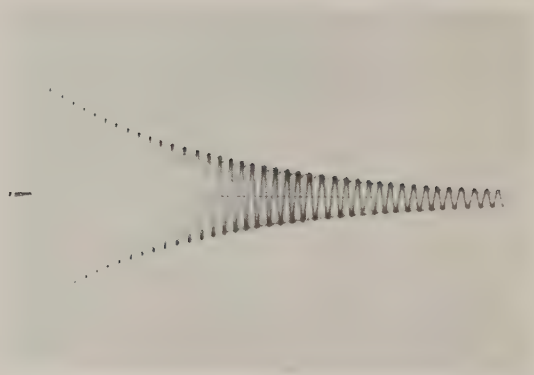


FIG. 3.—TYPICAL PLATE OF DECREMENT GAUGE.

[*To face page* 81

or breaking. At the centre of this fibre, and at right angles to it, are fixed two 10μ fibres 2·5 cms. long, one on either side of the suspension. The ends of these remote from the frame are fused into a sphere about 50μ in diameter, while the other ends are fixed together by a minute amount of iron, which when activated by an electro magnet outside the case also serves to start the oscillations of the cross-piece, the damping of which is measured. If the external case is of glass, the frame is held in the cylindrical socket D by the spring E, while if it is of silica the frame is fused directly to it. Two flat windows, G and F, are fused to the case, the former for observing and the latter for illuminating the tip of the cross-piece. The gauge is connected to the other apparatus at I, communication with the interior being through the hole H.

The advantages of this gauge are :—

(1) The period of oscillation of the cross-piece is of the order of 1 sec., which is much longer than that of the usual type of quartz fibre gauge, in which a single

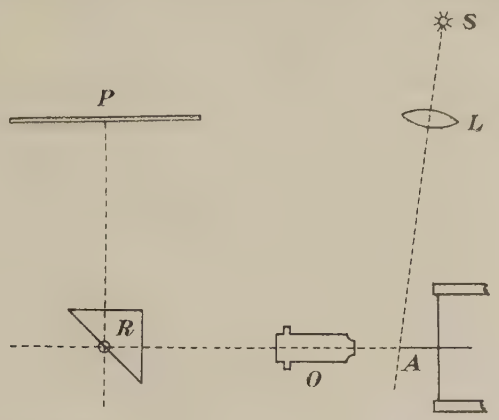


Fig. 2.—Diagram of Optical System for Decrement Gauge.

fibre about 100μ thick and some cms. long is clamped rigidly at one end and the oscillations of the free end observed. Hence, instead of taking the time for the amplitude of the oscillations to fall to a certain fraction, and so calculating the damping, a record of all the oscillations can be obtained photographically. This was done by the method shown in Fig. 2. Light from the lamp S is focused by the lens L on to the tip of the pointer A. This acts as a convex mirror of very large curvature, and by means of the microscope objective O a small image of the lamp is formed on the photographic plate P after reflection in the right-angled prism R. This prism is slowly rotated during the oscillations by a clockwork motor driving through a worm and cog, with the result that damped oscillation curves of the type shown (Fig. 3) are produced on the plate.

(2) The second advantage of the gauge is that the plane of vibration cannot change as in the usual quartz fibre gauge,* and thus the true decrement is always obtained. If by chance the cross-piece oscillates in itself, the fact is easily discernable on the plate.

(3) The zero is fixed and the oscillations are not disturbed by external vibrations

* Zeits. f. Electrochem., 20, 296 (1914) ; Phil. Mag., 47, 32 (1914), and other Papers.

as in the decrement gauges of Sutherland* and Hogg,† or the Knudsen gauge in any of its various forms.‡

(4) The gauge, especially the type with a silica case, contains very few metal parts, the presence of which may give rise to considerable absorption of gas at low pressures.

The theory of the gauge is the same as for all types of decrement gauge—viz., the relation between the damping and the pressure is given by the equation

$$d=KP\sqrt{\frac{M}{T}}$$

where d is the damping, P the pressure, M the molecular weight of the gas present, T the temperature and K a constant depending on the dimensions of the instrument. In the case where several gases are present, the sum of a number of terms must be taken corresponding to the partial pressures of the various constituents, and we have an equation of the type

$$d=\frac{K}{\sqrt{T}}\left(P_1\sqrt{M}_1+P_2\sqrt{M}_2+\ \ldots\right)$$

The instrument is not absolute, but must be calibrated against known pressures. These were obtained by connecting it to a large volume (about 2,000 c.c.), in which small bulbs containing a known amount of gas could be broken by electromagnetic hammers, thus producing known small pressures. These bulbs varied in volume from 0·1 to 0·01 c.c., and, having been accurately calibrated, were filled at a pressure of between 3 and 5 cms. of mercury, which pressure could be measured with a mercury monometer with a very small error.

In calibration, the whole apparatus was exhausted and heated to about 300° C. for some hours till it became quite hard. Pure dry oxygen was then let in to about 2 cms., and after being allowed to stand over night it was pumped out without extra heating. This method was adopted, since the baked out walls of the glass were found to absorb the gas during calibration, while if they were saturated with the gas which was about to be used this phenomenon did not occur. The apparatus was then sealed off from the pumps and the bulbs containing oxygen broken one by one.

When the apparatus is fully exhausted there is still a small damping d_0 of the oscillations, due to the residual gas and the actual damping of the quartz fibres, which reduces the amplitude to half value after about 500 complete swings. Since this is always about the same, the latter cause is probably predominant. The figures of a typical calibration with oxygen are as follows :—

$P\sqrt{M}10^4$ P in mms. of Hg	d	$d-d_0$	$\frac{d-d_0}{P\sqrt{M}}$
0	1·00104	.....	.....
14·60	1·00173	0.00069	0·472
34·58	1·00262	0·00158	0·457
54·99	1·00369	0·00265	0·482
76·37	1·00463	0·00359	0·472
100·77	1·00580	0.00476	0·472
125 50	1·00710	0·00606	0·482
150·69	1·00822	0·00718	0·476

When these points are plotted they all fall practically on a straight line (Fig. 4).

* Phil. Mag., 43, 83 (1897).

† Proc. Am. Acad., 42, 115 (1906), and 45, 3 (1909).

‡ Ann. Phys., 44, 525 (1914) ; Ann. Phys., 41, 1 (1913), and other Papers.

Fig. 5 gives points obtained for another instrument, when calibrations were ade with three different gases—oxygen, argon and carbon dioxide—and shows at the $P\sqrt{M}$ law is true. The relation was linear for this gauge for values of $\sqrt{M}$ up to about 0·125 [P in cms. of Hg], which corresponds to a pressure of

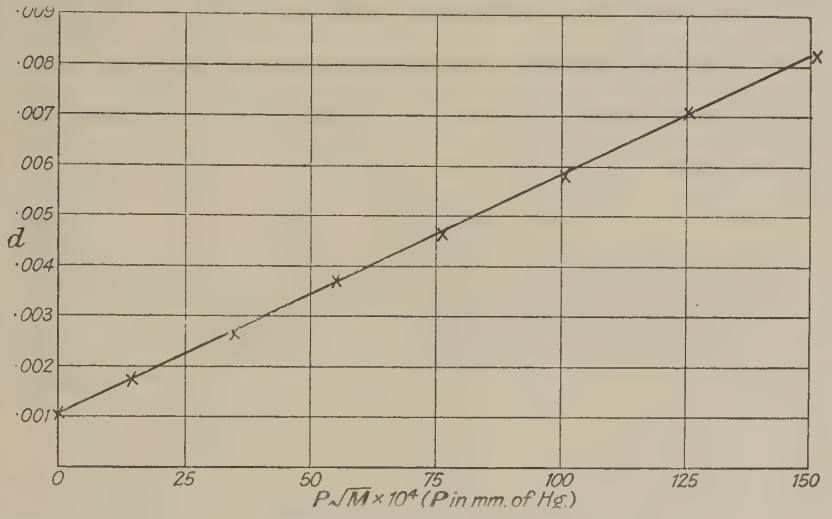


Fig. 4.—Calibration Curve of Decrement Gauge.

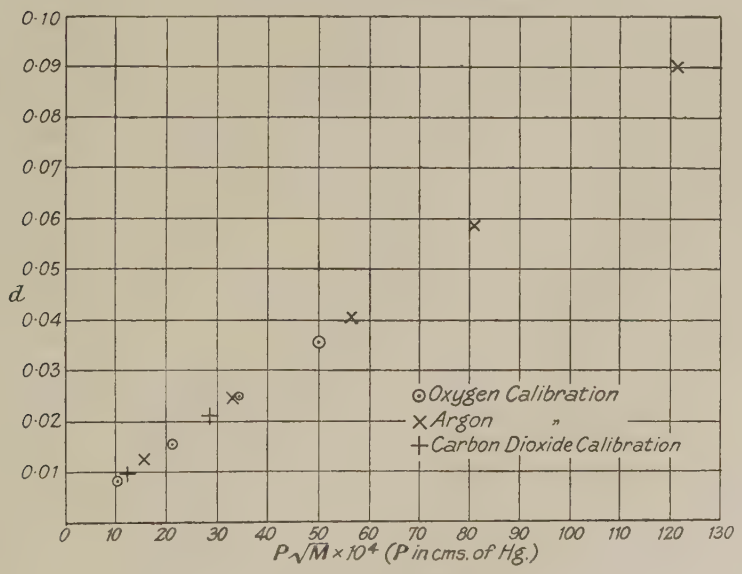


Fig. 5.—Calibration Curve with Various Gases.

xygen of $2 \cdot 10^{-3}$ mm. The lower limit of the gauge for light gas, such as oxygen, s $2 \cdot 10^{-6}$ mm., while for a heavy gas, such as mercury, is $1 \cdot 10^{-6}$ mm.

Owing to the fact that any metal in a gauge may absorb or give out gas, thus nvalidating results, and also for use with active vapours, it is desirable to have a

gauge which contains no metal exposed to the gas. For this purpose a modification of the Pirani gauge* has been made. This gauge depends for its action on the fact that, as the pressure changes the heat loss from the hot filament to the walls changes. Thus, if the current in the filament is constant, the temperature and therefore the resistance changes. In the modification described the filament is replaced by a spiral glass capillary tube *A* (Fig. 6), whose internal diameter is from 10 to 20μ, external diameter from 100 to 200μ, and total length, when unwound, about 100 cms. This spiral ends in the larger tubes *BB*, which are held together by the glass rods *CC*, which hold the whole rigid when it is being sealed into the outer case. The spiral and the tubes *BB* are then filled with mercury and form a conducting filament with a glass surface. The resistance of the gauges made varies from 300 ohms to 1,700

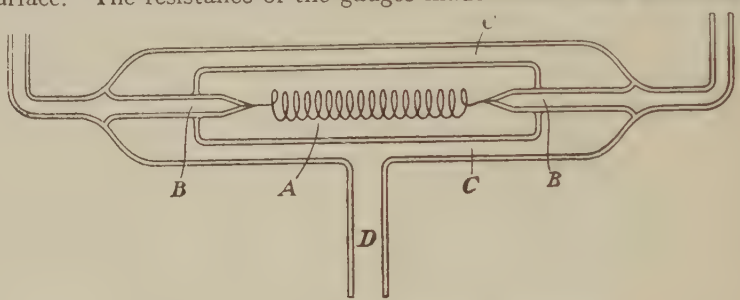


FIG. 6.—DIAGRAM OF MODIFIED PIRANI GAUGE.

ohms. In practice the gauge is placed in one arm of a Wheatstone bridge, which is balanced when the filament is at about 30°C. above the temperature of the case, which is kept in a thermostat. The voltage which must be put across the bridge in order to keep it balanced is then used as a measure of the pressure. These gauges have not yet been carefully calibrated, but a typical one of 1,562 ohms resistance, with the filament at 44°C., needed 15 volts across the bridge at a pressure of about 0·2 mm. To keep the bridge balanced at a pressure of about $5 \cdot 10^{-4}$ mm. this potential had to be reduced to 5 volts. The sensitivity would thus seem to be as great as other Pirani gauges, and equilibrium is reached within 30 seconds of switching on the current. It is hoped that a fuller account of these gauges may be given later.

In conclusion I must express my deep gratitude to Prof. Lindemann for originally suggesting the two gauges, and to both him and Mr. T. C. Keeley for their continual help and advice.

NOTE ADDED DECEMBER 1, 1925.

In the latest decrement gauge with a glass case the cylindrical socket *D* is replaced by a fused silica-glass joint. The metal spring is then not needed.

DISCUSSION.

Dr. G. W. C. KAYE: Mr. King has shown us two or three devices which are likely to be of value to the worker on high vacua. In his first device the author has developed a neat technique for recording the damping by what might be described as an adaptation of Threlfall's gravity balance. The method of measuring the pressure in an enclosure by sharing with it the contents of "capsules" of gas of known amount is a useful means of avoiding the use of the McLeod gauge. As regards the mercury-in-glass-filament Pirani gauge, I wonder whether one of the most useful features of the metal-filament Pirani gauge—that of very quick response—would not be largely nullified. In my own work with metal-filament lamp Pirani gauges connected to a discharge tube I have had trouble from electrostatic effects, and I imagine that these effects would still persist with the author's type of instrument.

* Trans. Am. Electrochem. Soc., 20, 243 (1911).

XI.—AN APPLICATION OF SPECTRO-PHOTOGRAPHY TO THE MEASUREMENT OF HIGH TEMPERATURES.

By I. O. Griffith, *M.A.*

Read at Oxford July 4, 1925.

Abstract.

The spectrum of the source is photographed through a neutral wedge placed immediately in front of a photographic plate. The curved boundary of the spectrum varies in height, the variation depending upon the distribution of energy in the source—i.e., upon its temperature. It is assumed that the source radiates as a black body or a grey body, but with certain limitations the method is applicable to bodies which are neither black or grey.

IN this note is described an application of photography to the accurate determination of the temperature of a luminous body. The temperature of such a body can be determined from a knowledge of the ratio of the radiation intensities corresponding to any two wavelengths in its spectrum. Various methods are available for measuring these intensities, but for a source which is difficult to maintain in a steady state for any length of time, such as the one which the writer has been investigating, a photographic method, permitting an almost instantaneous record of the spectrum of the source, is extremely convenient.

If for a source at temperature T, E_λ denotes the intensity of radiation of wavelength λ falling on the receiving instrument, in our case a photographic plate, we may write

$$E_\lambda = a_\lambda \cdot \varepsilon_\lambda \cdot \varphi(\lambda, T) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (1)$$

a_λ is the absorption coefficient for wavelength λ of the various lenses and prisms which the radiation traverses in its passage from the source to the plate, and ε_λ represents the emission coefficient of the radiation for wavelength λ. For black or grey body radiation, ε_λ is a constant depending on the degree of greyness of the body and $\varphi(\lambda, T)$ represents Planck's function.

For another wavelength λ'

$$E_{\lambda'} = a_{\lambda'} \cdot \varepsilon_{\lambda'} \cdot \varphi(\lambda', T)$$

and

$$\frac{E_\lambda \cdot d\lambda}{E_{\lambda'} \cdot d\lambda'} = \frac{a_\lambda \cdot \varepsilon_\lambda \cdot \varphi(\lambda, T) d\lambda}{a_{\lambda'} \cdot \varepsilon_{\lambda'} \cdot \varphi(\lambda', T) d\lambda'}$$

If we have a comparison source at a known temperature, T_s, then

$$\frac{S_\lambda \cdot d\lambda}{S_{\lambda'} \cdot d\lambda'} = \frac{a_\lambda \cdot \varepsilon_\lambda \cdot \varphi(\lambda, T_s) d\lambda}{a_{\lambda'} \cdot \varepsilon_{\lambda'} \cdot \varphi(\lambda', T_s) d\lambda'}$$

where S_λ denotes the radiation intensity for wavelength λ of the comparison source, a carbon arc at atmospheric pressure.

Hence
$$\frac{E_\lambda d\lambda}{E_{\lambda'} d\lambda'} \div \frac{S_\lambda d\lambda}{S_{\lambda'} d\lambda'} = \frac{\psi(\lambda,\lambda',T)}{\psi(\lambda,\lambda',T_s)} \quad \ldots\ldots\ldots (2)$$

where $\psi(\lambda,\lambda',T)$ has been written for $\dfrac{\varphi(\lambda,T)}{\varphi(\lambda',T)}$

The dispersion $d\lambda$ and absorption α_λ of the apparatus intermediate between the source and the receiving instrument have been eliminated. It will be noticed that the emission characteristic of the source represented by ε_λ has also disappeared, so that the method is applicable to the determination of the temperature of non-black bodies.

For the measurement of the ratio of radiation intensities of various wavelengths, the photographic intensities as measured by the density of the photographic image are reduced to absolute intensities by means of a neutral wedge,* as suggested by Merton and Nicholson.†

The light from the source is arranged to fall on the slit of a spectograph, so that its intensity in a direction parallel to the length of the slit is constant. In front of the spectrometer slit or in contact with the photographic plate is placed the neutral wedge with its lines of equal density parallel to the dispersion of the spectograph, the absorption of the wedge increasing uniformly in a direction parallel to the slit.

The law of absorption of the wedge is given by
$$E = E_0 \cdot 10^{-Ky} \quad \ldots\ldots\ldots (3)$$
y being the distance measured parallel to the slit between two points corresponding to intensities E, E_0 in the light transmitted through the wedge. The value of the constant K which has been found convenient in practice is 1·5 or 3 per centimetre, according as the wedge is near the photographic plate or in front of the spectrometer slit.

The image of the source as it appears on the plate is shown in Fig. 1, the intensity along any ordinate decreasing uniformly in density from top to bottom. The curved boundary of this spectrum varies in height, as measured from the reference line, the variation depending upon the distribution of light in the source, the absorption in the apparatus including the wedge, the dispersion of the spectograph and the colour sensitivity of the plate. Every plate is standardised by photographing on each plate the spectrum of the comparison source.

After development, the image densities are measured and the distances from the zero line to the points where the density has a certain fixed value are recorded, so that for any given wavelength we know the value of y corresponding to a certain value of $\log E$ and
$$\log E = \log E_\lambda - Ky \quad \ldots\ldots\ldots (4)$$

* The wedge consists either of a wedge-shaped piece of neutral grey glass cemented to a similar piece of clear glass to form a parallel plate, or of a very thin wedge of neutral grey gelatine between glass plates.

† Phil. Trans., A, Vol. 216, p. 459, and Phil. Trans., A, Vol. 217, p. 237. During the course of the investigation a Paper by Plaskett has been published [publications of the Dominion Astrophysical Observatory, Victoria, B.C., Vol. 11, No. 12], in which a similar method is described in connexion with work in astronomical spectrophotometry.

FIG. 1.

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In the same way, for the comparison source which is recorded on the same plate

$$\log E = \log S_\lambda - Ky_s \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$\therefore \log \frac{E_\lambda}{S_\lambda} = K(y - y_s) \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

It will be noticed that no characteristic of the plate or of its treatment enters into this equation, and that E_λ and S_λ are measured in terms of a constant intensity E, under the one condition that equal intensities of light of the same colour give equal densities on the same plate.

The actual values of y measured are the distances from the reference line, but as we are only concerned with differences, the fact that the reference line may not actually be the line of zero density of the wedge does not matter.

We may write equation (2) in the form

$$K(y - y_s) - K'(y' - y_s') = \log \frac{\psi(\lambda, \lambda', T)}{\psi(\lambda, \lambda', T_s)} \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (7)$$

The y ordinates are measured on a photometer designed by Mr. G. M. Dobson,* in which the sensitive element is a photo-electric cell connected through a high resistance to a sensitive quick period string electrometer.† Variations in the source of illumination and in the voltage applied to the photo-electric cell are eliminated. The density of the background or unexposed parts of the plate is automatically allowed for, and all densities can be measured with equally good accuracy.

As the light does not necessarily fall on the wedge as a parallel pencil, it is essential that the measurement of the wedge constants K and K' should be carried out when the wedge is in its actual position in the instrument. A constant source of light—viz., a tungsten filament focus bulb—is placed in the position previously occupied by the luminous source whose temperature is being measured. The intensity is reduced in a definite ratio by interposing in the parallel part of the beam a perforated gauze, which is rotated in order to eliminate the effect of any irregularities in the perforations. Two photographs of the source are taken on a single plate, one with and one without the perforated gauze. After each exposure and before the plate is removed from the spectograph, a helium tube is placed in front of the slit and the lines of its spectrum serve as reference lines. Within the spectral range which is utilised (4,000 Å. to 7,000 Å.) the values of K are constant—i.e., the wedge is neutral—and in practise a mean value of K of 1·46 per cm, has been taken as correct over the effective portion of the spectrum, the wedge being placed in contact with the plate.

The y ordinates of an image such as that shown in Fig. 1 can be measured with a probable error of 1/20 mm., so that the probable error in the expression on the left-hand side of equation (7) is 1/10 mm. This corresponds to a probable error in temperature of approximately 40° at 5,000° and 150° at 10,000°.

* Proc. Roy. Soc.

† Lindemann and Keeley, Phil. Mag., 47, March (1924).

XII.—ON THE BENDING OF CRYSTALS.

By C. H. Bosanquet.

Read at Oxford, July 4, 1925.

IT was observed by Coblenz in 1903* that slips of rocksalt immersed in strong brine at 50°-70° could be plastically bent. This phenomenon was mentioned by R. W. Wood at the British Association meeting in 1923, and has recently been investigated by Joffé. Experiments in this laboratory show that slips about 15×5×1-2 mm. can be bent into a semicircle in hot water or hot brine.

The distortion may be due to one of three causes :—

(*a*) There may be irrotational slip along cleavage planes, all portions of the crystal remaining parallel to their original direction.

(*b*) There may be alteration in the spacing of the crystal lattice, as in the elastic bending of a rod.

(*c*) The large crystal may break down into a number of small undistorted crystals, each of which makes a small angle with its neighbours. In this case the prismatic spaces between crystals would presumably be filled with NaCl in an amorphous state.

As regards the first possibility, of irrotational slip, the ends of the specimen appear to remain perpendicular to the other faces in their vicinity, instead of parallel to each other, and this is also true of the general direction of cleavage if a specimen is broken in half. The broken ends are not in general flat, but quite appreciably cylindrical.

If the second supposition is true, and bending entails changes of the interatomic distances without change of neighbours by the individual atoms, it is difficult to see why the slip should not straighten elastically as soon as the forces are removed.

The third possibility, that of breaking down into undistorted microcrystals, thus appears most likely, and it is also favoured by the curvature of the fractured surfaces.

These theories have been tested by X-ray reflexion. In Fig. 1 *C* is the crystal, *P* the plate, *S* a lead screen with a wide slit. The anticathode acts sufficiently well as a line source without the use of any narrow slits. *DB* is the direct beam throwing a shadow of the crystal on the plate, and *RB* is the reflected beam. During an exposure the crystal was rocked through an angle of about 10° to ensure that all portions passed through their reflecting angle.

Owing to the small area of the reflecting faces and the large angle through which the crystals were rocked, most of the reflexions, though perfectly clear on the negatives, were too faint for reproduction, and all the figures are taken from tracings of the negatives.

* Phys. Rev. (1903).

Fig. 2 shows the α and β lines of Mo reflected from a curved crystal, and the position of the axis of rotation is shown dotted. If the curvature were due to irrotational slip, the reflected lines should have exactly the same size and shape as the upper edge of the shadow, whilst actually they are at least twice as long. This indicates that the orientation of the lattice varies from one part of the crystal to another.

Fig. 3 (A and B) are tracings of two reflexions from broken ends of curved

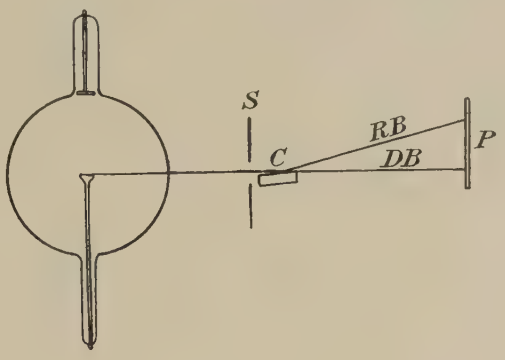

Fig. 1.

crystals; A was so faint that only the α line was visible. The fact that the reflected lines are arcs, instead of short lines of length about equal to the diameter of the anticathode, shows that considerable variations of orientation occur among the microcrystals in a single cross-section, as was to be expected from the shape of the fracture. The arcs also are portions of circles with centres at the shadow of the

Fig. 2.

reflecting face, indicating that within the limits of measurement, or rather less than per cent., the longitudinal spacing of the atomic planes is the same along both inner and outer edges of the curve. With the same accuracy the radii of the arcs agreed with the distance between direct and reflected beams obtained with an unbent crystal.

In fact, no crystal gave any reflexion which indicated a change in the lattice constant, in spite of the fact that the radius of curvature was only of the order of five times the thickness.

Fig. 4 shows the effect with an unbent crystal. Owing to the fact that the lines were not lengthened by distortion, the intensity is much greater in this case, and all four lines in the K spectrum of Mo appear. The length of the lines is entirely accounted for by the geometry of the apparatus.

The crystals used in the photographs of Fig. 3 were also rotated through 90°, so that reflexions were obtained from their edges, the plane of incidence being roughly

Fig. 3.

parallel to the tangent planes of the curved surfaces. The results were indistinguishable from Fig. 4, showing that planes parallel to the plane of bending remain sensibly unwarped.

Fig. 5 shows the effect of limiting the width of the beam by means of a vertical slit. Only the shaded portion of the crystal was illuminated, and the shortness of the lines indicates that the microcrystals over a small area of the face are sensibly parallel to each other.

These conclusions are in general agreement with the work of Joffé.* He

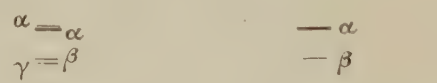

Fig. 4. Fig. 5.

is of the opinion that the process of bending consists of the breaking down of the crystal into small units, which slip along rhombododekahedral planes, and in addition turn through various angles. He makes no suggestion, however, as to what is the substance filling up the irregular spaces left between the crystals.

* Zeit. fur Phys., 22.

The slip, then, behaves as if it consisted of a number of small perfect crystals embedded in a very viscous fluid, presumably amorphous NaCl. The action of the hot liquid in bending is probably the prevention of the formation of cracks. If a minute fissure forms it will immediately be filled with NaCl solution saturated with respect to plane surfaces, and supersaturated with respect to concave surfaces, so that solid will be deposited in the crack at the point of maximum stress. The result of this will be that the tensile strength will be enormously increased. Joffé and Lewitsky * find that the tensile strength of a certain specimen was 450 g. per square mm. dry, and more than 4,000 in water, the elastic limit being 920 g. per square mm. The tensile strength when dry is independent of temperature, and the elastic limit decreases with temperature until they become equal at about 200°.

These results all appear to be in accordance with the theory outlined above.

* Zeit. fur Phys., 31 (1925).

DEMONSTRATION OF THE KINETIC PROPERTIES OF A GAS JET.

By Dr. J. S. G. Thomas.

THE demonstration was designed to illustrate the Bernoullian property of a gas jet, which has a central region of low pressure. Gas discharged through a large orifice from a gas cylinder was employed.

(*a*) The jet was directed downwards on to a plate, and it was shown that a ping-pong ball, instead of being blown away when placed on the plate, takes up a position of stable equilibrium at the centre of the jet. The stability was demonstrated by inclining the plane on which the ball was supported: a considerable inclination fails to dislodge the ball. When brought near the orifice the ball flies up to this and adheres to it.

(*b*) The jet was directed upwards and a toy balloon thrown into it. The balloon then rested in stable equilibrium at a considerable height above the jet, and the equilibrium persisted even when the jet was inclined to the vertical. On insertion of the operator's hand into the jet so as partly to shield the balloon, the latter appeared to be attracted towards the hand; this phenomenon afforded an illustration of Le Sage's theory of gravitation.

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